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PAPERS intended for the Medical Transactions, may be sent *to the Registrar of the College of Physicians, in Warwick-Lane.*

ERRATA CORRIGENDA.

- Page 29, line 4, *for* lorqu' *read* lorsqu'.
- 33,——16, *for* recueillis *read* recueillir.
- 34,—— 9, *for* reunie *read* réunie.
- 35,——13, *for* cérébrale *read* cérébrale.
- 23, *for* eblouir *read* éblouir.
- 25, *for* attribue *read* attribué.
- 36,——17, *for* degre *read* degré.
- 38,——12, *for* l'ecorce *read* l'écorce.
- 13, *for* reunie *read* réunie.
- 39,—— 2, *from* bottom, *for* mora *read* morâ.
- 40,—— 8, *for* magna *read* magnâ.
- 22, *for* compositæ *read* composita.
- 2, *from* bottom, *for* althæa *read* althæâ.
- 44,——10, *for* ammoniacæ *read* ammoniæ.
- 47,—— 7, *for* æris *read* aëris.
- 48,——11, *for* probam *read* probationem.
- 12, *dele* in.
- 49,—— 6, *for* hac *read* hâc.
- *for* liquidem *read* liquidum.
- 359,—— 3, *from* bottom, *for* 3^{ss.} *read* f 3^{ss.}

M E D I C A L

T R A N S A C T I O N S.

- I. *History of a Case of Strangulated Hernia, successfully treated by the Application of Ice, after the Attempt at Reduction by the Taxis had failed.* By G. D. YEATS, M. D. F. R. S. Fellow of the Royal College of Physicians, &c. &c.

Read at the COLLEGE, November 1st, 1817.

Dublin Castle, March, 1807.

FELLSEE Nicholson, æt. 18, Groom to the Hon: Coll. Seymour, was seized on Monday morning, March 16th, 1807, with sickness and vomiting, accompanied by great pain in the right groin and back. He was, at the time, going with a pail to the pump for water for his horses, and when he was returning with it the pain increased so violently that he was obliged to request another person to carry it for him. This happened between seven and eight o'clock; and about half past nine I was desired to see him. I found him with a considerable swelling in the right groin,

VOL. VI. B protrud-

protruding into the scrotum, evidently produced by a strangulated hernia of the congenital kind. It was extremely painful to the touch and very tense; there was great soreness over the abdomen, more particularly to the right side, and very slight pressure produced much pain. The countenance was pale and sunk; the pulse 100, full and hard; and much thirst had come on. He could not lie in a horizontal, nor keep himself in an erect, posture, as such situations seemed to stretch the tumor, as he expressed it, and caused pain, undoubtedly by tightening the integuments, and thus producing greater pressure on the painful hernial protuberance. He was obliged to sit with his body bent forwards at a somewhat acute angle with his thighs, and thus to relax the whole of the abdomen. Twelve ounces of blood were taken from the arm, from which great relief was obtained, and he expressed himself easier as the blood flowed. Twelve leeches were
7 applied

applied to the tumor and the blood was encouraged to flow, by fomentations, for a considerable time. He then took five grains of the submuriæ hydrargyri with one of opium, and one grain of the former was ordered every hour after. As soon as the blood from the leeches had ceased to flow, ice, broken into small pieces, was applied to the tumor, and confined there by surrounding it with a twisted towel, leaving an open circular space, like a well, that more might be supplied as it melted. A person was left with him to apply it unremittingly. In the afternoon a purgative glyster was administered, which brought away some fæces. The pain was by this time mitigated, and the inflammation and tumor were certainly less, so that he could lie horizontally with comfort. I now requested Mr. Macklin, State-Surgeon to His Grace the Lord Lieutenant, to visit him; and notwithstanding

that gentleman is well known for his surgical skill, the reduction of the hernia by the taxis was attempted without success. Very great pain and soreness were induced, so that he was obliged, after an effort of some time, to desist from further attempts to return it into the abdomen. A grain of opium was directed at night, and the submuriat and ice continued as before. On the following morning the pulse was calmer and the countenance improved ; but he had passed an indifferent night, on account of the increased pain from the attempt at reduction, and he had also been very sick. The tumor, however, continued of diminished bulk, and the sore tension of the abdomen was almost entirely gone. The same remedies were given, and another purgative glyster ordered. In the evening the glyster had produced a fæculent evacuation, and by bed-time the hernia had returned into the abdomen
with

with the testicle also. The testicle descended into the scrotum again on the following day, and on examination was found to be much smaller and softer than the other. He took in the whole about eighteen grains of the submuriat, but his gums had not become sore. As soon as the hernia had returned he had two or three spontaneous fæculent evacuations. While he laboured under the disease he complained of severe griping pains within the abdomen, shooting into the tumor, arising, I should think, from the stimulus of the mercury inciting the peristaltic motion of the intestines to propel their contents through the obstructed part, where the insuperable impediment caused the more violent pain. Soon after the ice had been applied the pain remitted, and the part felt benumbed. About eight years previous to this hernial attack, when riding on horseback, he was thrown on the pummel of the saddle
and

and bruised his testicle, which became so much swelled and inflamed that he was confined to his bed for several weeks. Since this accident a tumor had occasionally appeared in the groin, but never at any time to give him so much distress, nor to be of such a size, as in the present instance.

Cases are upon record in which the application of æther and other matters producing cold has effected the reduction of the strangulated hernia, but like all other remedies indiscriminately used, they have failed of success. It will not be doubted that the frequent forcible handling of a strangulated hernial tumor adds to the mischief, in as much as the strong compression by the hand, in the attempts at reduction, forces into a smaller space that which is already too much straightened by a dangerous confinement of the parts. Upon similar grounds it is that I think the indiscriminate and unassisted application of cold has not only proved

proved fruitless, but under some circumstances, I should fear, injurious. The effect of the application of cold is, to diminish the bulk by constricting the parts and lessening distension, as far as it can be done by the abstraction of heat; but it appears to me, that this mere constriction of parts, without the quantity of the contents of the vessels be diminished, will not so well answer the intended purpose; for the bulk of the hernia will be increased by other causes than by the contents within the intestine itself, it being manifest that the arterial impulse will drive the blood into the vessels contained within the sac, when it will not pass off by the veins in which the momentum is much less, hence there is a constant supply without a corresponding waste. If the condition be not relieved, by hastening the exit and by diminishing the supply, the period will speedily arrive when no more blood can be passed into

into the tumor from the parts being in a state of full congestion ; and as every part of the body must be constantly visited by moving blood to be in a state of health, it will follow that this stagnation soon ends in the loss of vitality or gangrene. If the local inflammatory tension be diminished, it will allow the venous blood, in part, at least, to pass the strangulated part. The propriety, therefore, of general bleeding, to lessen the force of the circulation, and of the local abstraction of blood, to diminish the bulk of the tumor and to arrest the morbid action of the small arteries, becomes apparent. It has frequently happened indeed from this increased congestion of the arteries not finding a ready vent by the vasa afferentia, that dropsical collections have taken place in hernial sacs. I do not think it ought to be expected that immediate great relief from painful sensations should be felt by general bleeding in strangulated

gulated hernia, for the diminution of the force of the blood thereby cannot very speedily affect the circulation or bulk of the incarcerated contents within the hernial sac. We should not, however, altogether deny therefore the ultimate good effects of such an evacuation in these cases, for whatever diminishes the morbid force of the circulation must lessen the impulse of the blood to the hernial tumor. We know that in ordinary severe inflammations of the bowels, instantaneous relief is very often obtained by a large bleeding, but here circumstances differ; the circulation is comparatively free in the inflamed part, consequently the abstraction of blood produces an immediate and direct effect, there being no confinement of the vessels by mechanical compression, as in hernia: and probably the speedy relief from pain in the present case, was obtained by the effect of the bleeding on the inflammation without the tumor. In
proportion

proportion to the quantity of intestine protruded into the hernial sac will be the quantity of blood confined; this will in the same degree diminish the quantity of returning blood to the right ventricle of the heart, and will so far act as a bleeding upon that cavity. Under such circumstances, therefore, large and repeated bleedings cannot have a good effect; nothing can be so beneficial as the reduction of the size of the tumor to let loose the circulation there, for if the largest possible quantity of blood were taken from the general circulation, it would not alone avail in subduing the disease. I would not, therefore, altogether reject general bleeding, as some have recommended*, nor would I indulge in it so largely as in inflammatory congestions, when there is no impediment to the returning blood†. If the bulk of the tumor be diminished

* Cooper, Wilmer, Alanson.

† Pott, Richten, Callisen.

by local bleeding, as also the momentum of the circulation by the general abstraction of blood, then the application of cold becomes a powerfully successful agent. The cold too will lessen by its sedative power the action of the smaller arteries, for although the general bleeding will diminish the *vis a tergo* of the blood, it will in many instances have no effect upon the local action of the arteries, which will go on to act morbidly until death arrests their action ; because you may weaken the impulsive force of the general circulation without lessening the action of the small arteries : this is an important fact which cannot be too often impressed upon the mind. The effect likewise of the constant application of cold, is to cause a suspension of the active life of the parts ; they therefore collapse from torpor, and thus the hernia returns into the abdomen. The quantity of
of

of blood taken from the arm was not great, but the local depletion was considerable, and was designedly encouraged for several hours. After this, with the assistance of the ice, the tumor was found diminished and less painful. I thought it then the proper opportunity to request the aid of the surgeon, but the reduction was not effected; and it was our joint opinion that perseverance in the attempt would only increase the danger.

A consideration of the nature of strangulated hernia, as well as experience, justifies us in abstaining from purgatives given by the mouth, until it is found by a diminution in the size, pain and tension of the tumor and abdomen, that the obstruction is beginning to yield, and even then it is not often necessary to have recourse to them, for generally as soon as the hernial sac is relieved of its load, by the return of the protruded parts into the

the

the abdomen, evacuations spontaneously occur. It is most adviseable to wait until the hernia is entirely reduced before purgatives are given, because there is danger of hurrying the contents of the bowels into the sac, and reproducing the strangulation before it is completely subdued. The administration, however, of laxative glysters are obviously required; but I am inclined to believe that the frequent exhibition of these, especially when made with acrid ingredients, is hurtful, from the pain which has been complained of above the obstructed part when they have been exhibited, arising from increased peristaltic motion being produced there by sympathy. I direct the submuriat in small doses of a grain frequently repeated, with a view to its relaxant effect, which it powerfully exerts when it affects the constitution, instead of employing it as a purgative, the irritation of which in the obstructed part only increases the

the

the mischief *. Opium was therefore occasionally given, not only to obviate the irritation of the submuriat upon the intestines, but to assist by its antispasmodic power : and I may here mention, although I cannot now urge the reasons in detail, that I have almost uniformly found the combined exhibition of submuriat of mercury and opium, one of the best means of arresting the progress of inflammation, especially under circumstances when that morbid condition continues after repeated general bleeding, and when it seems to bid defiance to all you can do, by lessening the general momentum of the blood. In ileus,

* By relaxant power, I mean the destruction of that tensive state of the circulation, both locally and generally, accompanying all highly inflammatory diseases, and which experience has fully convinced me, small doses of the submuriat, properly managed, very frequently effects. The submuriat obviates too that engorgement of the organ under inflammation, which often by continued irritation wears out the patient.

acute rheumatism, pleurisy, and all purely inflammatory diseases, this combination, not omitting, of course, depletion, has pretty faithfully served me.

II. *Some Observations upon Paraplegia in Adults.* By MATTHEW BAILLIE, M.D. F.R.S. L. and E. *Fellow of the Royal College of Physicians, &c. &c.*

Read at the COLLEGE, November 1st, 1817.

THE most common form of paralysis in adults is that called hemiplegia, in which one side of the body has lost more or less of its nervous energy. But there is another form of paralysis in adults called paraplegia, in which the lower half of the body is more or less impaired in its nervous power. This last form of paralytic affection has, as far as I can judge from individual experience, increased considerably in this country within the last fifteen or twenty years, although it be very difficult to assign any satisfactory reason for it.

Paraplegia in adults has been considered by most medical men as being produced by some disease, either in the bones or ligaments of the spine

spine or in the cavity of the spine, most commonly at the loins, independently of any disease in the brain. The reason of this general opinion has probably been, that the lower part of the body from the loins downwards is in this disease affected with paralysis, and that in children a similar disease is often obviously dependent upon a morbid affection of the lumbar, or some other portion of the spine. In adults, however, where there has been no accident affecting the spine by outward violence, paraplegia, I believe, depends most commonly in a great measure upon a disease affecting the brain itself. This opinion I have entertained for several years, and some other medical men have likewise held the same opinion*. It is how-

* Mr. Copeland has briefly mentioned this cause of paraplegia in an excellent Treatise upon the Symptoms and Treatment of the diseased Spine, p. 10. Sir Henry Hallford, Sir James Earle, and some others have also entertained this opinion.

ever by no means general; and the chief object which I have in view in writing this paper, is to render this opinion more commonly known, that it may either be established or be properly limited by the future observations of other practitioners.

In the paraplegia of adults it rarely happens that any disease can be detected by the most careful examination in the lumbar part of the spine, or in any other part of it. It may be said, however, that in such cases the disease may exist within the cavity of the spine. This may certainly be the case without any external disease in the spine, and I have no doubt that it has sometimes occurred. The dura mater may be thickened within the cavity of the spine, or a serous fluid may be effused there, or blood may be extravasated there, or a tumor may be formed there, which will press upon a part of the spinal marrow, or a portion of the spinal marrow may
itself

itself undergo some morbid change. Each of these causes may occasionally exist, and I have no doubt that all of them have occasionally produced paraplegia, without the smallest external disease in the spine, and without any morbid affection of the brain. I believe, however, that these causes of paraplegia much more rarely occur than has commonly been supposed, and that this disease most frequently depends upon some cause of pressure acting upon the brain itself. In such cases the diseased condition of the brain may immediately affect a portion of the spinal marrow, and its nerves, in the same manner as it affects the nerves of a lower limb in hemiplegia; or if there be any effusion of serum between the membranes of the brain, which is a very common occurrence, a portion of the serum may fall into the cavity of the Theca Vertebralis, and press upon the lower part of the spinal marrow.

Paraplegia in adults may take place at an early period of life, but more commonly occurs at the middle or a more advanced age; it also occurs much more frequently in men than in women. Upon inquiry into the symptoms, some affection of the head will generally be discovered, either some feeling of pain in it, or giddiness, or sense of weight, or undue drowsiness, and the vision is often more or less impaired. These symptoms exist in very different degrees in different individuals, but it seldom happens that none of them exist, and that the head is altogether free from some uneasy feeling. Sometimes the sight of one eye is almost entirely lost, and its pupil appears dilated as in gutta serena, and occasionally there is a paralytic dropping of the upper eyelid of one eye. Sometimes the affection of the brain is marked by a defect in the memory, and a want of the ready exercise of the general powers of the mind.

mind. Sometimes one or both of the upper extremities are affected more or less with numbness, and with a feebleness in their motions, when no disease whatever can be found in the cervical part of the spine. These circumstances afford strong evidence that the cause of the disease exists in such cases within the cavity of the scull, and that it consists in some mode of pressure upon the brain. With respect to the symptoms of paraplegia which have been more commonly taken notice of, they consist in a sense of numbness in the lower limbs, with an impaired motion in them. At first there is only the appearance of some stiffness or awkwardness in the motions of the limbs, but in time the want of power of motion in the limbs and the inability to preserve the due balance of the body very much increase, and the person cannot walk without the assistance of one or two sticks, or the aid of some other

other person, who more or less supports him. As the disease advances, the urine passes off more and more in a feeble stream, and at length often passes away involuntarily. The bowels in this disease are almost always costive, and at length, from a want of power in the sphincter muscles of the anus, the motions frequently pass off unrestrained by the will. Patients in this complaint will often live for many years, but most commonly the symptoms gradually increase, and at the end of a few years they die with their constitutions entirely exhausted. In a few instances recovery takes place.

I have not had much opportunity of becoming acquainted with the morbid appearances in this disease, but I shall relate what were found upon a very careful dissection in one of the most strongly marked cases of paraplegia which I have ever known.

The bones of the scull, more especially at the sutures, were more vascular

cular than usual; the dura mater presented nearly its natural appearance, but the vessels of the pia mater were very much loaded with blood, and there were effusions of serum between the different membranes of the brain on both sides of it. The tunica arachnoides was opaque and much thickened. The substance of the cerebrum was considerably firmer, and that of the cerebellum was considerably softer than is natural. About four ounces of water were found in the lateral ventricles of the brain, and a considerable quantity of water was discharged from within the theca of the spinal marrow*. In this case the diseased appearances of the brain were very strongly marked. In different cases I entertain little doubt that

* The account of this examination was communicated to me by Mr. Pennington, of Montague Place, who had attended the patient for many years. About this patient I was occasionally consulted.

different appearances will be found, for the causes of pressure upon the brain may be very various ; but there are few instances of a morbid change in the structure of the brain which are not attended with an effusion of water between its membranes, and also into the lateral ventricles. A portion of the water effused between the membranes of the brain may escape into the cavity of the theca vertebralis, and compress the spinal marrow at its lower extremity*.

I do not know any very successful method of treating this disease, but that which I have found the most successful is chiefly applicable to it, as depending upon some pressure on the brain. This consists in taking away blood by cupping from the nape of

* I think it not improbable that in those cases of paraplegia in which the brain is found to be diseased, the morbid affection extends to both hemispheres of the brain, but this point can only be ascertained by a great number of dissections.

the neck, in the application of leeches to the temples, in applying a blister to the nape of the neck, and in inserting a seton there. The internal remedies which I have used with most advantage are calomel, or the *pilula hydrargyri*, combined with squills, together with purgative medicines. A grain of calomel, or five grains of the *pilula hydrargyri* with one grain of dried squills, have been directed to be taken every night for many weeks. The purgative medicines have consisted chiefly of neutral salts, and sometimes of pills composed of the *extractum colocynthidis compositum*, with an addition of jalap.

I have in a few instances found considerable advantage from the lower limbs being rubbed with the hand for an hour twice a day. In one case I found that a good deal of benefit was derived from electric sparks being drawn from the lower limbs. From tepid bathing, both in fresh and sea water,

ter,

ter, I believe that considerable advantage has been derived in some cases of paraplegia, but I have not in my own experience found either application very useful. In cases of paraplegia, where the cause has evidently been a disease in some of the bones or ligaments of the spine, or in its cavity, independently of a morbid affection of the brain, a considerable part of the above treatment is not applicable. The same observation likewise should be made with respect to those cases of paraplegia which are connected with the influence of lead upon the functions of the stomach and the bowels, in which the Bath waters are often eminently useful. To treat of either of these cases of paraplegia, does not fall within the scope of the present paper.

III. *Observations Medicales.* Par Jos. ROM. LOUIS KERCKHOFFS, *Docteur en Médecine, Médecin de l'Armée des Pays-Bas, dirigeant le Service de Santé Militaire à Ruremonde, Assesseur étranger de la Société de Minéralogie de Jena, Membre de la Société Suédoise de Médecine séante à Stockholm, de la Société libre d'émulation et d'encouragement pour les Sciences et Arts, de Liege, &c. &c. Communicated by the Registrar.*

Read at the COLLEGE, November 1st, 1817.

S. I.

Plique Polonaise.

LA plique polonaise, (*plica polonica, trichoma*). Cette affection des cheveux est décrite comme endémique en Pologne, et on la caractérise par leur entrelacement, leur agglutination, leurs saignements quand on les coupe ; et par l'accompagnement d'une puanteur des cheveux, d'une odeur forte et dégoutante qui s'exhale du corps par la transpiration, de vertiges, de violentes céphalalgies, d'un tintement et d'un écoulement fétide d'oreilles, de l'affaiblissement de la vue, et enfin d'une prostration

prostration générale des forces prononcée, dont les phénomènes le présentent quelquefois sous la forme la plus variée.

Cette affection mérite de fixer l'attention des médecins, par rapport aux absurdités et aux opinions controuvées, dont sa description fait, tour-à-tour, l'objet dans les ouvrages-mêmes de quelques auteurs justement célèbres, sans vouloir aller jusqu'à réfuter les contes ridicules, qu'en ont fait des superstitieux aveuglés par un stupide fanatisme.

Quelques hommes de l'art la regardent comme contagieuse ; d'autres lui refusent ce caractère, et la supposent héréditaire ; d'autres n'y voient qu'altération des humeurs : en considération de ce conflit d'erreurs et de tant d'autres débitées sur la plique, et que je passe sous silence, je crois obliger mes confrères en leur transmettant les observations, que j'ai eu occasion de faire à ce sujet, pendant
mon

mon séjour en Pologne, où j'ai été avec les armées Françaises, auxquelles j'appartenais en qualité de Médecin, lorsqu'elles allaient à Moscou.

Je désire que mes observations ramènent, à l'égard de la plique, vers le chemin de la vérité les pathologistes qui s'en sont éloignés. Le lecteur s'étonnera indubitablement de ce qu'elles ne sont pas tout-à-fait d'accord avec ce que ont dit de savans Médecins Polonais. Qu'on me permette de remarquer, qu'il n'est pas surprenant, que ceux-ci aussi bien que d'autres s'en soient écartés ; c'est que les mauvais raisonnemens, suite d'un faux jugement avec lequel on procède trop souvent dans la carrière de l'art de guérir, donnent nombre de fois lieu à des idées les plus erronées, qui, en se repandant gagnent de crédit et finissent d'être applaudies par les esprits les plus justes.

Ayant attaché mes recherches, d'une façon spéciale, à l'étude et à l'examen
de

de la plique, j'ose dire avec assurance, qu'elle ne provient absolument que de la malpropreté, réunie à l'habitude dans laquelle est une grande partie de la basse classe des Polonais, de porter les cheveux longs et d'avoir la tête continuellement couverte, soit d'un épais bonnet de laine, soit d'une calotte de cuir. Ce qui vient à l'appui de mon assertion, c'est que les riches sont en général exempts de la plique. Elle se manifeste ordinairement parmi les pauvres qui croupissent dans la misère, surtout parmi les Juifs, ces hommes si négligeants pour la propreté.

Aux environs de Poosen, dans un petit village extrêmement pauvre, j'ai vu la première plique. Un garçon de 15 à 18 ans en faisait le sujet, il était d'une saleté affreuse, couché dans un trou obscur et puant à côté des bestiaux ; il avait les cheveux noirs, très longs, très gras, entrelâchés par touffes (depuis un an passé suivant le dire de

ses parens) ; il avait la tête couverte de crasse, le cerveau fortement affecté en se plaignant particulièrement de maux de tête terribles. Le Médecin, qui traitait le malade et que j'accompagnais, demanda mon opinion : je répondis que je n'y voyais que malpropreté. Je proposais de faire respirer au malade le grand air et de lui couper, peu-à-peu, les cheveux. Contre ce dernier article, mon collègue séduit par le préjugé vulgaire, par lequel ce moyen est exclu comme très dangereux, s'y opposa, en exhibant le danger le plus éminent ; parce que le cerveau, repliqua-t-il, est accablé d'une grande irritation, en coupant les cheveux on augmentera cette irritation, on fera fluer les humeurs dans une proportion trop considérable vers ce viscère, et il en résultera l'apoplexie ou autre affection cérébrale mortelle. La raison qu'il m'objecta, quelque fondée qu'elle fut, ne put m'empêcher de l'engager, de tout mon possible,

possible, à procéder à l'exécution de ma proposition. Elle fut adoptée, sauf que mon collègue, pour toute sûreté, fut d'avis d'administrer un laxatif, afin d'exciter l'excrétion dans le canal intestinal et d'établir ainsi un foyer de fluxion pour les humeurs*. Un laxatif composé de rhubarbe et de soufre sublimé fut donné. La largeur de deux doigts à peu près fut enlevée aux cheveux ; le prétendu saignement ne se montrait pas. Deux jours après, la même chose fut répétée en tenant toujours la tête convenablement chaude. Quatre jours après, même répétition. En continuant ce procédé, le malade avait, au bout d'une vingtaine de jours, tous les cheveux coupés sans qu'aucun accident ne fut survenu ; alors il commençait à se peigner un peu, et à se laver légèrement la tête avec du lait : quelques amers et autres toniques lui

* *Qu'on ne me juge pas humoriste. Je me sers des expressions de mon collègue.*

furent prescrits, et il ne tardait pas à jouir de la meilleure santé.

J'ai profité de cette occasion pour me convaincre, dans la croyance que j'étais, que la plique n'est aucunement contagieuse. J'ai inoculé sans le moindre effet, d'abord sur moi-même ensuite sur deux enfans, des bulbes de cheveux, que j'avais arrachés à ce garçon.

Au moment que je fus arrivé aux voisinages de la Vistule, où la plique, d'après les écrits des gens de l'art, se rencontre le plus, je m'adressai aux différens Médecins du pays, afin d'en recueillis tous les renseignements possibles. Ils s'accordaient à me communiquer que depuis quelques années elle était devenue fort rare. J'ai attribué cela à ce qu'on commençait à porter les cheveux plus courts qu'autrefois. Plusieurs de ces Messieurs, m'assurant avoir eu à traiter des personnes attaquées de cette affection, tombaient d'accord avec moi sur sa

cause occasionelle ; mais d'autres me soutenaient que la malpropreté seule n'y donnait pas lieu, puisque le peuple d'une foule de contrées, disaient-ils, sont aussi malpropres que les Polonais, sans toutefois être sujets à la plique. Pour les combattre, je n'avais qu'à leur demander si ailleurs la malpropreté était aussi réunie à l'habitude de porter les cheveux longs et de couvrir continuellement la tête d'un épais bonnet, comme le font les Polonais.

Dans un hameau près de Calvary, j'ai eu lieu de confirmer l'idée que j'avais conçue de la plique sur un juif, chez lequel logeait un Pharmacien de l'armée Française, qui m'avait prévenu du cas. Un chirurgien de campagne traitait le malade ; il m'a conduit auprès de ce malheureux juif, qui était âgé de trente à quarante ans, d'une saleté inexprimable, dans une position égalant presque celle du garçon précédent.

Le

Le marasme dans lequel était cet individu ; sa barbe hérissée et crasseuse ; sa physionomie sinistre et égarée ; ses ongles longs et ressemblants à des griffes qui, comme on le racontait, n'avaient pas été coupés pendant deux à trois années ; tout cela formait un aspect hideux et effrayant.

L'intrication de ses cheveux, qui étaient noirs, très longs, très gras, considérablement sales, avait déjà existé deux ans. Elle était accompagnée d'une forte affection cérébrale et de la fièvre hectique, dont les symptômes, suivant le rapport du chirurgien traitant, ne s'étaient déclarés que depuis huit à neuf mois. Quoique des auteurs d'une certaine réputation, probablement entraînés et imposés par un pareil cas, exposent que la fièvre hectique, est fréquemment une affection symptomatique de la plique, je ne me suis point laissé éblouir. Je ne l'ai regardée que comme accidentelle ; j'ai attribué sa cause occa-

sionelle à la même que celle de la plique, c'est-à-dire au défaut de la propreté. Ce n'était également que de la négligence de cette vertu domestique, d'où provenait l'état des ongles, que quelques Médecins auraient pris, mal-à-propos, pour un symptôme de la plique ; car ce ne peut être que d'un semblable état accidentel qu'est venu l'erreur de dire, que pendant son cours, il survient maintefois un changement dans les ongles aux doigts et aux pieds, qui deviennent longs et épais comme des cornes, il en est demême, ce me semble, de la chute des cheveux, symptôme d'un degré avancé de la fièvre hectique et qui a été vue sur le juif. Ce ne peut être qu'une affection accidentelle de cette dernière nature, qui a induit en erreur les hommes de l'art, qui ont envisagé comme un symptôme particulier et mortel de la plique le cas où les touffes tombent d'elles mêmes.

Pour

Pour terminer notre histoire il nous reste à remarquer, que la fièvre hectique parvenue à un point, où il n'avait plus aucune espérance de guérison, annonçait une mort très prochaine. Dans le but de pallier, je proposais un traitement tonique et de couper les cheveux. Ma proposition fut accueillie. Le malade fit usage d'un vin généreux, des bouillons restaurans, et d'une potion composée du quinquina, de la valeriane, du lichen d'islande et de l'extrait de pissenlit. Les cheveux furent coupés graduellement en conservant d'une chaleur naturelle la tête. Au bout d'une vingtaine de jours, les cheveux étant enlevés sans qu'aucun accident n'en ait suivi, on lavait la tête avec une décoction de guimauve. Par ce procédé thérapeutique, le malade a encore trainé sa triste existence dix à onze mois, conformément à ce que m'a informé le chirurgien-traitant.

S. II.

S. II.

Phthisie Muqueuse.

Dans cette maladie asthénique, dont la cause prochaine est le relachement de la membrane muqueuse, qui tapisse les voies aëriennes, j'ai obtenu sur deux individus, sur lesquels cette espèce de phthisie avait fait tant de progrès que le pronostic était entièrement désespéré, des succès admirables de l'administration de la poudre de l'écorce de Saule (*Salix alba Linn.*) reunie au soufre sublimé en forme d'électuaire fait avec le sirop de pavots, en donnant sur six parties du Saule une partie du soufre.

Les malades faisaient usage, en même tems, d'un bon vin de Bourdeaux, des bouillons analeptiques, des alimens farineux.

Le Médecin Leurs, de Ruremonde, m'a rapporté que sa pratique, qui est extrêmement étendue, lui a fourni
différentes

différentes fois l'occasion, de constater dans le traitement de la phthisie muqueuse les bons effets du guy de chêne (*viscum album Linn.*) employé en forme de décoction ou en poudre. A ce titre, ce remède trop négligé de nos jours, mérite, d'après les observations du Dr. Leurs, d'occuper un rang distingué dans la matière médicale.

S. III.

Relatio Militis acetate plumbi mortui.

Vigèsimâ quintâ Aprilis anni millesimi octingentesimi decimi sexti circa horam septimam vespertinam accersitus fui à Domino Ridder, secundæ classis chirurgus agminis quadragésimi sexti militiæ nationalis, ad Joannem Bax, tympanotribam centuriæ quintæ ejusdem agminis, ut examinarem hujus individui dolores.

Sine mora eò me contuli et hominem mortuum inveni. Statim observavi,

servavi, ventrem ejus esse vehementer durum ac corpus integrum palloris subflavi.

Omnibus de statu hujus tympanotribæ investigatis, doctus fui, vigesimâ tertiâ Aprilis eum fuisse solito pallidiorem, et queri incepisse de constipatione, de anorexiâ, de magna extremitatum lassitudine ac de generali virium defectione.

Vigesimâ quartâ hæc symptomata augescebant.

Vigesimâ quintâ sentiebantur colicæ, quæ ingeminabantur cum sensatione suffocationis, regressu ventris introrsum, nauseis, horrendis convulsionibus, sudore frigido-viscoso, aphoniâ, trismo. Horâ secundâ hujus diei postmeridianâ Chirurgus Ridder appellatus fuit ad ægrotum: fomenta calida ex hyoscyamo ac floribus chamæmeli compositæ abdomini applicari jubebat, eodemque tempore potionem decocti ex althæa cum melle præscribebat: æger solummodò dimidium

dium cochlearis exsorbêre potuit causâ contractionis maxillæ: post aliqua temporis momenta quasdam guttulas tincturæ succini cum æthere et tincturâ opii crocatâ, eodemque tempore clisma ex floribus chamæmeli, radicibus althææ ac melle præparatum, imperabat: agrotans nec eis uti potuit. Hâc eâdem die morti occubuit.

Diversa symptomata, quibus victima laboravit, mihi dederunt illicò locum reflectendi de veneficio per plumbum.

Vigesimâ sextâ autopsia cadaveris horâ septimâ vespertinâ.

Invenimus stomachum summopere inflammatum, ejus vasa fortiter impleta, membranam mucosam pluribus in partibus maceratam, præsertim circa pylorum.

Una pars æsophagi circa cardiam, intestinum duodenum, portio ascendens ac portio transversa coli, pancreas, pars mesenterii ac pars intestini jejuni, facies concava hepatis atque
splenis

splenis erant in statu inflammatorio evidenti.

Vesicula fellis erat magnopere impleta bili.

Collegimus in stomacho fere sex uncias liquoris fusco-nigri coloris : hic liquor collectus et in aquâ distillatâ extensus ac filtratione mundatus* habebat saporem subdulcem, metallicum ac parum adstringentem ; atque eum evaporando vapor acidi acetici agnoscebatur†.

Iste liquor per analysim chimicam phænomena mihi obtulit sequentia.

1°.

In hunc liquorem infusis aliquibus guttulis acidi sulphurici, alba præcipitatio exorta est : hoc præcipitatum, postquam requievit, erat pulvis albus,

* Prima operatio in tali casu agenda.

† Residuum remanens super filtrum erat pulvis cinereus, subflavus, qui permixtus cum hydrosulfureto potassæ sulfurato acquirebat colorem fusco-nigrum per præsentiam plumbi.

insipidus

insipidus et in aquâ distillatâ insolubilis, quique mutabatur in nigrum resplendens hydrosulfureto potassæ sulphurato; (*proprietas sulphatis plumbi*) ad evaporationem hujus pulveris processi atque illum in cochleari argenteo cum sebo miscui: factâ combustionem, reliquum microscopio examinavi, et globulos metallicos coloris plumbi reperi. Hoc residuum dissolutum in acido acetico ipsi dabat saporem subdulcem: hanc dissolutionem percolavi atque in aquam fontanam infudi; hæc turbulenta fiebat, acquirens colorem lacteum (*Liquor obtulit idem phænomenon.*)*

2°.

Liquori filtrato immiscui hydrosulfuretum potassæ sulfuratum et præcipitatio fusco-nigra visa est.

3°.

Liquorem conjeci in hydrosulfure-

* Acetas plumbi in aquam fontanam infusa hunc producit effectum.

tum calcis sulfuratum, et hæc præcipitatio erat parumper clarior quam præcedens.

4°.

Liquor injectus in dissolutionem carbonatis potassæ in aquâ distillatâ turbabatur cum præcipitatione albocinereâ, relicto præcipitato ejusdem coloris.

5°.

In liquorem infusâ dissolutione carbonatis ammoniacæ, nascebatur præcipitatio et præcipitatum coloris albocinerei.

6°.

Liquor colore viridi inficiebat syrupum violarum. Acetas plumbi hunc profert effectum.

7°.

In liquorem injecto acido sulfurico concentrato, distinguebatur odor acidi acetici.

8°.

8°.

Cum acetates igne decomponi possint, liquorem hoc medio tractavi, et pulverem coloris cinerei (*oxidum plumbi*) retinui, quem oxigenio per carbonem privavi, tunc reliquum examinari microscopio et vidi globulos metallicos.

9°.

Liquore jactato in dissolutionem potassæ puræ in aquâ distillatâ, provenit leve præcipitatum album, ac super hanc materiam fuso acido sulfurico, secuta est levis effervescentia ac odoris acidi acetici evaporatio.

Itaque symptomata morbi, autopsia cadaveris ac analysis chimica nullum poterant relinquere dubium, quin venenum esset acetas plumbi: attamen me continere haud potui ab instituendis, quæ possibiles erant, perquisitionibus, ut quamdam obtinerem dilucidationem de momento, quo toxicum illud in stomachum fuit introductum.

ductum. Post plures tandem dies certior factus sum, hunc infelicem tympanotribam, potius cupidine impulsus, vigesima secunda Aprilis vespere intravisse in cubiculum cujusdam suorum sociorum, qui utebatur extracto saturni contra gonorrhœam chronicam, ibique invenisse phiolam, hac compositione saturnina impletam, eamque exhausisse, deceptum indubitanter subdulci hujus medicamenti sapore.

S. IV.

Gangræna Nosocomialis.

Gangræna nosocomialis singulari modo meam excitavit attentionem tempore bellicarum expeditionum anni 1812, 1813 et 1814 : tunc temporis in exercitibus gallicis medicus occasionem habui continuo hanc observandi affectionem ; itaque de hoc morbo colligere potui magnum observationum numerum. Intime con-

victus

victus sum, eam pro causa occasionali habere eadem miasmata quæ efficiunt causam remotam febris adynamicæ, atque, ut illi occuratur, sufficere, ut observetur in cubiculis vulneratorum rigida mundities, atque permittatur, sine ulla intermissione, libera æris circulatio, tersa tenendo, in quantum possibile est, et corpus et vulnera.

S. V.

Scabies.

Numerus scabiosorum, quos pluribus ab hinc annis tractavi, mihi omnino experientiâ probavit successum, qui in scabie simplici contagiosa obtinetur ablutione infusionis arnicæ montanæ, in qua dissolvitur murias sodæ v. g. in duabus libris aquæ bullientis infunduntur uncia quatuor florum vel radicis arnicæ : hoc, unicæ tempore horæ, infuso fit colatura, in quâ dissolvuntur uncia duæ muriatis sodæ :

sodæ : hâc lavat scabiosus ter quarterque in die corporis partes, in quibus papulæ observantur.



S. VI.

Panaritium.

Pluribus abhinc annis usus fomentationum dissolutionis carbonatis potassæ in aquâ fervidâ, particulariter meam fixit attentionem in tractandis panaritiis.

Ingens observationum numerus, quas hâc in re praxis mihi suppeditavit, dedêre clàrissimi successûs probam, quem hoc topicum operatur in in phlegmonosâ hâc inflammatione, scilicet quando suppuratio se declaravit.

Meus processus, has administrandi fomentationes, est sequens.

Quando suppuratio panaritii se manifestavit, et quod incisio opportuna partis inflammatae facta est ; duæ
unciæ

uncia carbonatis potassæ in duabus tribusve libris aquæ bullientis dissolvuntur, et quum hujus temperatura descendit usque ad 40 aut 38 gradum (thermometri Reaumur), manus affecta in hac fovetur: si hoc liquidem desinit esse calidum, recalefacere oportet: et hoc modo hæc balnea localia singulis duabus horis repetuntur.

Absque eo quod examini subjiciatur utrùm hæ fomentationes agant per potassam et caloricum, utpote potentias in hâc inflammatione excitantes, quæ naturæ asthenicæ esse videtur, simul ac suppuratio se monstrat; sive hæ fomentationes agant ratione specificâ, factum est constans, experientiâ omnino convictus sum, topica stimulantia, maximè insignia, longè abesse, ut adeo desideratum producant effectum.

IV. *A Description of an unusual Appearance in the Viscera of an Infant, in which the Gall-bladder was wanting, in a Letter to the President of the Royal College of Physicians.* By HENRY JAMES CHOLMELEY, M.D. Fellow of the Royal College of Physicians, &c. &c.

Read at the COLLEGE, May 7th, 1817.

DEAR SIR,

THE inspection of an infant's body, that died under my care, presented an unusual variety, which may be worthy of being recorded in the Medical Transactions of the College of Physicians. I shall be very brief in the statement subjoined, as there are no pathological or physiological inferences to be drawn that are not sufficiently obvious.

The infant was observed by the grandfather (a medical man) on the day after birth to be very sallow, which appearance daily increased; and, in consequence of its becoming completely icteric, he requested my advice. The meconium passed was
scanty,

scanty, but of usual appearance. An emetic was exhibited, which caused the vomiting up of some dark green bile, and the yellowness of the tunica adnata and skin became somewhat diminished. The stools however still continued white and pasty, and the urine of a very deep hue. After this the child continued to be obstinately jaundiced; and at the end of five weeks it was attacked with convulsions that terminated its existence. The child took the breast freely during the short period that it lived, but vomited up very constantly what it took. Its bowels were seldom moved without medicine. No pain was produced by pressure upon any part of the abdomen, nor was any unusual fulness, tension, or tumefaction observed.

Upon inspecting the viscera of the belly, the liver was observed of natural size, but of a green hue throughout its substance, in consequence of

bilious discolouration. The usual depression was left for the gall-bladder, but this appendage was totally wanting, and there was situated in its place only a narrow impervious cord, about the size of the cystic duct, with a small knot at each extremity. The pancreas, where connected with the duodenum, was very hard and firm, and both felt and cut like a schirrous one ; it was enlarged to the size of a small marble. This hardness was distinctly felt, before the bowels were disturbed to examine the particular state of the viscera, &c. All the other parts were sound and healthy. It is almost needless to add, that the transmission of bile into the duodenum was prevented by the pressure of the pancreas upon the termination of the ductus comm. choled., which in this child was perfect. Morgagni mentions one instance in which the gall-bladder was wanting, but in that child there were two livers ; and Mr. A.

Cooper has met with an instance, where there was no duct. comm. choled. ; but the gall-bladder was perfect.

I remain,

Dear Sir,

Yours respectfully,

H. J. CHOLMELEY.

*To the President of the
College of Physicians,
Warwick Lane.*

V. *Remarks upon the Effects of a Warm Climate in Pulmonary Consumption, and some other Diseases.* By HARRY WILLIAM CARTER, M.B. in a Letter dated from Geneva, July 18, 1817. Communicated by RICHARD POWELL, M.D. Fellow of the Royal College of Physicians, &c. &c.

Read at the COLLEGE, May 7th, 1818.

Geneva, July 18th, 1817.

DEAR SIR,

SHOULD the following remarks appear to you worth notice, you will oblige me by procuring their insertion in the Medico-Chirurgical Transactions, or any periodical medical publication of which you may approve. I mention the Transactions, as I imagine that work to be the most respectable of its kind; of this however you must be a far better judge than myself. Should my remarks appear to you too insignificant for insertion in any medical publication, you will have the goodness to return them to my father, whom I have requested to transmit them to you.

you. My paper will, at all events, prove to you that I sometimes think of my profession.

I remain, dear Sir,

ever very faithfully yours,

HARRY WILLIAM CARTER.

To Richard Powell, M.D.

&c. &c. &c.

Remarks upon the Effects of a Warm Climate in Pulmonary Consumption, and some other Diseases.

IT will not be denied that pulmonary consumption is one of the most melancholy and most destructive diseases with which mankind is afflicted. It is moreover unfortunately one whose ravages extend themselves especially over the united kingdom: the consideration of which must therefore be peculiarly interesting to every British physician. The great mortality which it occasions, and its continual increase, render

render it the duty of medical men to study it with all the attention in their power, and to endeavour to appreciate the means which have hitherto been proposed either to cure or to palliate it, as well as to seek for new remedies. Now, no disease has been more frequently or more minutely described than pulmonary consumption, in all its stages ; none has afforded more ample opportunities for the examination of morbid appearances, for none has been more fatal, and in none perhaps have the morbid appearances been more distinctly marked ; yet in no disease have the effects of medical treatment proved more generally uncertain. The variety of remedies which have been employed, and the various modes of treatment which have been adopted at different times by different practitioners, are, I think, sufficient to prove how little pulmonary consumption, when once confirmed, is under the controul of medicine.

There

There is, however, one point upon which almost all physicians seem to be agreed : they almost all concur in the propriety of phthisical persons removing from our inclement and variable climate to a milder and more steady one. As I have had some opportunities of observing the effect of a warm climate in phthisis, I beg leave to offer a few remarks upon the subject. The advice given to persons labouring under affections of the lungs to remove to a warm climate is, I think, too indiscriminately given. It is not unfrequently pressed upon those who are in a stage of the disease too far advanced for any considerable or permanent benefit to be expected from the change ; and sometimes upon those, who are in a situation so completely hopeless, that neither climate nor any thing else can possibly be of service to them. It may be said that physicians have some excuse for not sending their patients to a warm climate

mate earlier than they usually do ; it may be urged that the progress of phthisis is so insidious, and that people are so unwilling to believe themselves consumptive, that medical men often do not see them till the disease is pretty far advanced ; and that, even if they do see them at its commencement, they do not venture to propose a journey to the South, lest they should alarm their patient. But, in Great Britain, where pulmonary complaints prevail to so melancholy an extent, where the predisposition to them may be traced in so many families, and where this predisposition, as well as that to some other diseases, is perpetuated and spread, by the imprudent carelessness with which marriages are contracted at the present day, the very first appearance of affection of the lungs should be attended to : and, provided there be any predisposition to consumption in the patient, and his circumstances be
such

such as to render a journey to the South practicable, a mild climate should be recommended without delay. We should not be afraid of alarming the patient: it is much better that he should be frightened than that he should be lulled into a false security; and that the disease should be allowed to proceed till it becomes no longer under the controul of medicine, and cannot be arrested either by climate or regimen. Not a moment should be lost; for, in incipient phthisis, a warm climate may be of great benefit, as I have had occasion to observe in several instances. It is here necessary to caution invalids against a mistake into which many of them fall, and lose their lives in consequence. Let them not imagine that a residence of one winter abroad will be sufficient to restore a person, whose lungs are affected, to health. It will often be found necessary that several years should be passed out of Eng-

3

land;

land ; and in some instances the patient, if he would prolong his life, must be content to abandon his native country altogether. This I mention, because I have known some patients who have expressed great disappointment at finding they did not get well in a very few months ; and others who, trusting to the improvement they have experienced in the first winter, have returned prematurely home, and have fallen a sacrifice to their imprudence. A gentleman, in whose company I travelled for some time, and who had evident symptoms of incipient phthisis, for which he had been sent out of England, got much better during a residence of a few months in Sicily and the Morea. He then returned home, but soon found that he could not remain there : he had recourse once more to a warm climate ; he went to Madeira, and he has now for more than three years enjoyed pretty good health. A lady, who had symptoms

symptoms of incipient phthisis, was advised to go to Lisbon ; she did go and got much better. Upon her return to England, however, the complaint became worse. She again went abroad, fixing upon Nice for her winter residence, and she derived much benefit from the climate. In another case, where hæmoptysis took place in consequence of a fall from a horse, and where, from the general habit of the patient, as well as from consumption having occurred in his family, there was reason to fear that phthisis might ensue, the timely removal to a warm climate, with regularity of living, seem to have prevented any serious consequences. The uneasiness of chest and spitting of blood have not indeed ceased entirely, though the former is less constant than it was formerly, and is troublesome after too great exertion or exposure to cold only ; while the latter occurs but seldom, and the quantity of blood spit
up

up is very trifling. In this case the patient has, I think, derived benefit from a mild climate, but he is not in a state to return to England. One or two more winters passed in the South of Europe will, in all probability, restore him, at least so far as to enable him to live in his own country with care and proper precaution*. But,

* In this case there is no cough whatever, and no fever; and the pulse is generally about 75; but it is immediately quickened upon any exertion. It is to be observed, that the patient is a very young man, and that he has been considerably weakened by his very rapid growth, and by a gonorrhœa, which continued upon him upwards of a year. This case partakes, perhaps, in some measure of the nature of what Dr. Wilson Philip calls dyspeptic phthisis, though its symptoms do not altogether correspond with those, which he has described in the *Medico-Chirurgical Transactions*, vol. vii. part 2. The chief derangement appears decidedly to exist in the stomach and bowels. The latter never act without medicine; and this want of due action in them seems to depend upon derangement in the functions of the liver. I hope to restore the healthy action of the bowels by a plan which I found successful in a case of a description

from all I have observed, it is only in cases of incipient phthisis that a warm climate is of any advantage. When the disease has assumed a marked character, when there is much cough with purulent expectoration, flushing, and a frequent pulse, and emaciation, I believe that climate never effects a

scription not unlike this. In the case to which I allude, the stomach and bowels were brought to a perfectly healthy state by a steady perseverance in a course of gentle opening medicines. I generally gave the pills, of equal parts of alöes and soap, recommended by Dr. Hamilton; but, whenever these appeared to lose their effect, which sometimes happened, I suspended their use for a while, and gave some other medicine, as the compound rhubarb pill of the Edinburgh Pharmacopœia, or the compound extract of colocynth. The patient had spit blood occasionally for some time, and several of his near relations had died of pulmonary consumption. As the stomach and bowels improved the pulmonic symptoms yielded; and the violent headaches, from which the patient had suffered greatly for several years, left him. I paid particular attention to this case for upwards of two years. The subject of it is now wholly free from any complaint, and has for some time been able to discontinue his pills.

cure,

cure, and very seldom prolongs the existence of the sufferer. Indeed, it is the opinion of some medical men, that a warm climate accelerates the fatal event, especially when the situation fixed upon has been upon the sea coast. Whether the sea air be too stimulating for diseased lungs I leave to others to determine; I only know that it was the remark of an intelligent medical man of Nice, that strangers affected by phthisis, who resorted to that place, at first experienced some relief from their symptoms, but soon after began to decline very rapidly. This gentleman had every motive for wishing foreigners to resort to Nice; yet so convinced was he of the climate being improper for patients labouring under confirmed phthisis, that he was employed in writing a book upon the subject. Nor is M. Risso, the gentleman to whom I allude, the only medical man who considers that a warm climate rather accelerates the death

death of such persons. I remember having heard the same opinion when I was at Lisbon. I have myself seen several persons who have been sent abroad after the symptoms of phthisis have become unequivocal; and, though I cannot assert that they have sunk under the disease sooner than they would have done had they remained at home, I am satisfied that they have reaped no benefit from climate; at least the benefit has been trifling, and not permanent. In the instance of a West Indian servant, who was with me, I think that the disease was rather aggravated during his stay at Nice. In that of a lady, whom I attended every day for two months, I never could perceive any improvement. The disease went on gradually increasing, until she sunk under it*.

* The death of this lady was, I must confess, somewhat hastened by a journey which she undertook towards the close of her malady, but she earnestly desired to quit Nice, and, it is to be ob-

A young lady, who, when she arrived at Nice, was able to ride out, was soon confined to her bed, and died in a little more than a month. Of the persons labouring under pulmonary consumption, who were at Nice last winter, six died in the space of four months ; and one or two, whom I left there, were in a state which precluded all hopes of recovery. No example occurred of a person in confirmed phthisis being restored to health, and I have heard of only one who got better*. But it may be said the climate of Nice is by your own account objectionable. It is certainly not entirely free from ob-

served, that her impatience to move arose from her finding that she had not gained ground there, but was, on the contrary, growing daily worse.

* This patient I never saw, but I learned from the physician who attended him, that he was far gone in consumption. At one period he was extremely ill, but he rallied sufficiently to be able to leave Nice in the spring. How he bore his journey to Marseilles I have not heard.

jection,

jection, and perhaps there may be some situations more favourable to consumptive patients. Madeira may be better ; but, as far as I can judge, the climate of Nice is in no respect inferior to that of Lisbon, or of Naples ; and it is preferable to that of Marseilles, or of Montpellier, at both of which places patients suffer great inconvenience from the Mistrale and the Bise, that is to say, the north and north-east winds. The productions of Nice prove how mild must be its temperature. Last winter was unusually mild ; so that, if a warm climate could ever be of service to persons in confirmed phthisis, its effects ought to have manifested themselves during that season. I shall presently have occasion to say more of the climate of Nice ; but what I have already said is, I think, sufficient to prove that it is not because there is any thing peculiarly unfavourable in the climate that persons decidedly consumptive do

not recover there ; but because climate has not the power to cure confirmed phthisis, and can seldom even check its progress. Were this fact once established, physicians would, I presume, cease to recommend a journey to the South of Europe to those who have passed the first stage of pulmonary consumption. But even admitting that the fact is not fully established, it will at least be admitted that the benefit to be derived from a warm climate in the advanced stage of the disease is very problematical. The chance of benefit appears to me to be too slight to compensate the trouble and anxiety and fatigue attending a long journey. I have witnessed the distress occasioned by travelling under such circumstances, and I have seen how much the symptoms have been aggravated by it. More mischief is done by the journey than the finest climate can repair ; so that, even if we ascribe to climate much more
than

than I am disposed to grant, its advantages will be dearly bought at the expense of a long and wearisome journey to the South. Besides, if the patient should struggle through the winter, he will be obliged to move again, and to seek a cooler situation as the hot weather approaches. Even to persons in health, travelling for a long time becomes irksome; what then must it be to an invalid, especially when we take into the account the accommodations which are met with upon the road, and which are at best but comfortless and wretched, for one who requires every convenience? It has often been proposed to phthisical persons to go by sea to France or Italy, under the idea that the voyage might be of service; or at least that it would be less fatiguing than a journey by land. Now there can be little doubt that a sea voyage often does good in the *early stage of* phthisis: the sea air seems to be serviceable in
these

these cases ; and the sickness, which almost always takes place, is perhaps more efficacious than that excited by medicine. But, in confirmed phthisis, I confess I have been hitherto unable to discover that a sea voyage does any sort of good. It seems doubtful whether the sea air is useful here ; and vomiting appears to be rather hurtful than advantageous : it exhausts the patient without alleviating his complaint. Besides, the discomfort of being on board ship to those who have not been accustomed to it, added to the uncertainty and tædium of a sea voyage, of itself renders it almost as bad as a journey by land.

In short, I think, that upon every consideration, persons in confirmed phthisis should never quit their own country. By quitting it they will lose many comforts ; they will be deprived of the attendance of their nearest friends, who alone will give themselves the trouble to watch over them,
and

and to anticipate all their wants ; they will be deprived moreover of the attendance of medical men in whom they can place confidence, for English people are very apt to be prejudiced against foreign physicians ; they will expose themselves to much anxiety and fatigue, and all this for a vague hope of recovery or of prolonging life, a hope which I firmly believe is hardly ever realized. If they remain at home they may live in a regulated temperature ; and, if life is to be prolonged by any means, it will be, I should suppose, by this. I believe that my opinion of the expediency of persons in confirmed consumption remaining at home, will be found to coincide with that of all those who have attended their relations and friends abroad. They are all convinced of the inefficacy of climate to remove or much alleviate this afflicting disease, when it is once fully established.

In

In incipient phthisis, as I observed before, a warm climate may unquestionably be of great service. I do not mean to deny that the early symptoms of the disease may be overcome, or that they sometimes are overcome, without the patient taking a journey to the South of Europe ; but the climate of the South is nevertheless favourable to their removal. And moreover, when a person is advised to go abroad for his health, he is put upon his guard, and is likely to take more care of himself than the little inconvenience he experiences would induce him to do, were he to remain at home. In the early stage of the disease too a journey may be of some advantage.

When it is once determined that a patient shall go to a warm climate, the next point is to fix upon the spot which he ought to select ; and this is a point of some difficulty, for an extraordinary diversity of opinion seems to prevail among medical men with respect to

to the situation which should be chosen by persons affected by pulmonary complaints. From all that I have heard of Madeira, I should conclude the climate of that island to be one of the finest in the world ; but many persons would look upon Madeira as little better than a place of banishment, and there is one serious objection to it. The invalid is, in a manner, shut up in Funchal, for the surrounding country is so exceedingly mountainous as to render it almost impossible to take exercise. Climbing the mountains, either on horseback or on foot, is much too fatiguing for one whose lungs are affected. In Spain, Barcelona and Malaga are highly spoken of ; and I believe that the climate of these places is delightful in winter. If an invalid can make up his mind to fix himself where he will see few of his countrymen, and will meet with little to interest or amuse him in any way, he cannot perhaps

perhaps choose a better residence than Barcelona or Malaga. It has long been the fashion to send consumptive patients to Lisbon ; but the weather there in winter is often cold and rainy, and from all I have heard, as well as from what I know of Lisbon, I should by no means recommend it.

France or Italy are the countries to which invalids, as well as other people, generally wish to go. Now, in the former, Marseilles, Montpellier, Hyères have been chiefly recommended. With regard to Marseilles, I cannot but consider it as an improper place for those who labour under any affection of the lungs. The sun is generally very powerful, while the Mistrale, or north wind, which blows very frequently, is piercing cold. The changes of temperature at Marseilles are sudden and great ; and it is precisely this variability of climate which phthisical persons seek to avoid by quitting England. The climate of
Montpellier

Montpellier is not better than that of Marseilles ; it is extremely variable, and the inhabitants are in consequence very subject to catarrhal affections. The Bise, or north-east wind, and the Marin, or wind from the Mediterranean, are distressing to phthisical patients ; the latter is said to be so loaded with moisture, that when it prevails the furniture and beds become excessively damp, and must be aired before they can be used with safety*. Hyères is probably the best residence

* The author of the “ Itineraire de l’Empire Français,” who is of course favourable to his own country, thus expresses himself with regard to the climate of Montpellier. “ Le climat de cette ville est extrêmement doux et temperé. L’automne surtout y est très beau ; mais la variation dans la température est la source de beaucoup de maladies catarrheuses, et les étrangers doivent prendre garde de ne pas changer de vêtemens à la légère. Le *Bise*, et le *Marin*, ou les vents du nord est, et de mer, affectent sensiblement les nerfs. Le *Marin* surtout est d’une humidité qui s’étend même jusque sur les lits, qu’il faut faire chauffer.”

in France as to climate, but there are many objections to Hyères. It is but a poor place, the accommodations are very indifferent, it affords no society ; and medical assistance, in which any confidence can be placed, is not to be procured. In short, Hyères has nothing but its climate to boast ; and the inconveniences attending a residence there will, I think, more than counterbalance the advantages of climate. The principal places which have been recommended in Italy, are Florence, Pisa, Rome, Naples, and to these we may add Nice, since the Var is now once more the boundary between France and Italy. As to Florence, it is decidedly a bad winter residence for persons whose lungs are affected. The weather is generally chill, and there is much rain. Of the climate of Pisa I have little experience ; and, owing to the variety of opinions which have been given respecting it, I am totally un-

able

able to form any judgment, whether it deserve the high commendations which some have lavished upon it. At some future time I hope to be able to say more of the climate of Pisa. The climate of Rome is not to be compared with that of Naples, which is indeed delightful. I was at Naples during the months of February and March, 1815, and the weather was uniformly mild and pleasant. The evenings were rather cool, but a consumptive person would of course avoid being out after sun-set. Almost the only objection to Naples is the Scirocco, the south-east wind, which in summer is certainly very oppressive, and indeed almost intolerable; but in winter it is, I think, very bearable. I do not however know whether an invalid would be of my opinion. The scenery around Naples never ceases to be interesting; and the general fineness of the weather, and facility of conveyance from one place to another,

ther, enable an invalid to procure much instruction and amusement without risk or fatigue. Upon the whole, I think Naples a very eligible winter residence for a person whose lungs are affected. Yet, if the extraordinary interest of the environs of Naples be put out of the question, I must give the preference to Nice above every other situation in Italy with which I am acquainted*. The beauty of the territory of Nice, and the excellence of its climate, cannot be better described than in the words of Ludovico Rivelli. “ *Est Nicenus ager, licet exiguus, fertilitate tamen fertilissimus, aquarum inundantium irriguus, ac omnium arborum genere consitus; soli fertilitate, pabuli ubertate, situs salubritate, ac temperie, benignoque ven-*

* It has been already observed that Nice seems to be an improper place for patients in confirmed phthisis, (the same may be said of Naples, Lisbon, &c.) but the objections to it in such cases do not apply to incipient phthisis.

torum afflatu undique præpollens." The winter I passed there was certainly unusually mild; it was mild throughout Europe; but some judgment may be formed of the difference between the climate of Nice and that of the South of France, by comparing the state of the weather in either situation about the beginning of April. At Nice there were high winds, and what we considered bad weather at that time; yet the atmosphere was clear, and the thermometer was generally as high as 57° at about nine o'clock*. In the South of France

* From January 12th to May 6th, there occurred only three days of incessant rain, and four days when there were some showers. From March 7th to May 6th, the day I left Nice, I made regular observations on the temperature. It was as follows:

March.	Temp. 8½. o'clock.	Temp. 12 o'clock.	Weather.	Wind.
7.	59° .	59° .	Fine.	WSW.
8.	61° .	$63\frac{1}{2}^{\circ}$.	Fine.	WSW.
9.	59° .	$61\frac{1}{2}^{\circ}$.	Fine.	
10.	59° .	—	Fine.	

there were at the same period cold cutting winds, and, as I learned from

March.	Temp. 8½ o'clock.	Temp. 12 o'clock.	Weather.	Wind.
11.	57°.	57°.	Fine.	WSW.
12.	57°.	58°.	Fine.	
13.	57°.	59°.	Fine.	
14.	59°.	—	Fine.	
15.	57°.	—	Fine.	
16.	58°.	—	Fine.	
17.	54°.	54½°.	Cloudy.	
18.	54½°.	57°.	Fine.	
19.	57°.	—	Fine.	
20.	57°.	—	{ Fine day : rain at night.	
21.	54½°.	—		
22.	52°.	—	Showery.	
23.	56°.	—	—	
24.	57°.	—	Fine.	
25.	57°.	—	Fine.	
26.	57°.	—	Fine.	
27.	57°.	—	Fine.	
28.	58°.	—	Fine.	
29.	57°.	—	Fine.	
30.	56°.	—	Fine.	
31.	58°.	—	Fine.	
April.		2 o'clock, p.m.		
1.	58°.	61°.	Fine.	NE.
2.	57°.	61°.	{ Fine, much wind. }	NE.
3.	58°.	—		

persons who were travelling in that country, these winds were so violent

April.	Temp. 8½ o'clock.	Temp. 2 o'clock p.m.	Weather.	Wind.
4.	60°.	—	Fine.	
5.	59°.	—	{ Fine, high wind. }	NE by E.
6.	60°.	—	Fine. do.	do.
7.	59°.	—	Fine. do.	do.
8.	59°.	—	Fine. do.	do.
9.	59°.	—	Fine.	
10.	59°.	—	Fine, high wind.	
11.	58°.	—	Fine, do.	
12.	54½.	—	Fine, but cold.	
13.	54½.	—	Fine.	
14.	56°.	—	Fine.	
15.	57°.	—	Fine.	
16.	57°.	—	Fine.	
17.	57°.	—	Rain and Wind,	
18.	55°.	—	Fine.	
19.	55°.	—	Fine.	
20.	55°.	—	Fine.	
21.	54½.	—	Fine.	
22.	57°.	—	Cloudy.	
23.	57°.	—	Fine.	
24.	58°.	—	Fine.	
25.	58°.	—	Fine.	
26.	59°.	—	Fine.	
27.	59°.	—	{ Fine. Showery towards evening. }	

that the atmosphere was quite darkened by dust, and it became almost impossible to proceed. Compare the productions of the neighbourhood of Nice with those of the South of France and of Italy in general. The orange flourishes no where in France but at Hyères. The caruba, or locust-tree, the aloë, the Indian fig, the palm-tree, the pepper-tree flourish luxuriantly at Nice. The olive of France, though its fruit be excellent, is small and stunted ; and in no part of Europe, excepting Sicily, have I found it so large and handsome a tree as it is in the neighbourhood of Nice. In

	Temp. 8½ o'clock.	Temp. 2 o'clock p.m.	Weather.	Wind,
April.				
28.	57°.	—	Fine.	
29.	57°.	—	Fine.	
30.	57°.	—	Fine.	
May.				
1.	56°.	—	Cloudy.	
2.	57°.	—	Fine.	
3.	59°.	—	Fine.	
4.	59°.	—	Fine.	
5.	59°.	62°.	Fine.	

a word, the climate of Nice appears to me to be peculiarly fine. For the society of the place much cannot be said ; and perhaps it is better that an invalid should be removed from the temptation to go into company. The walks and rides about the town are as varied and pleasant as can be desired ; and, to one who has any knowledge of botany or geology, the country can never cease to be interesting. Whatever residence a phthisical person may fix upon, there are several precautions which he should carefully keep in view ; the neglect of which will do him more harm than a fine climate can do him good. In the first place, he must not trust to the mildness of the climate, and throw off any clothing to which he has been accustomed. He must not be induced by the softness of the air to be out late in the evening. One of the disadvantages which attaches itself to all the South of Europe, is the great difference of

temperature in the sun and in the shade ; and an invalid should be very careful not to expose himself to the sun, and afterwards stand in the shade by way of cooling himself. When the winds prevail, which are so common in the South, the patient should keep within doors. With regard to diet he ought to be very cautious. The wines he will meet with are very pleasant, and not heady, like our own, but they are obviously improper for persons whose lungs are affected. The high seasoned dishes which are so universally served up at tables abroad, and in such great variety, are to be shunned, as well as the sauces of which the French and Italians are so fond, both because they are too stimulating, and because their variety and high flavour induce people to eat more than is consistent with prudence. The fruits of the South, such as oranges, grapes, &c. are exceedingly wholesome and refreshing, and they
may

may be taken freely, but rather before than after dinner, since, taken after a sufficient meal, they only load and oppress the stomach.

Provided these precautions be attended to, a warm climate will, I think, hardly fail to be of benefit in incipient phthisis. It is to be observed, however, that neither Naples, nor Nice, nor any of the places which I have been mentioning, are proper residences for any body in Summer. Some of them become extremely unhealthy, and all of them are too hot, at that season. In the early part of the summer, perhaps, the banks of the Lago Maggiore, or of the lake of Como, or some spots in Tuscany, or Grasse in France, might be selected ; but in July, and the two following months, the heat is too great even at these places ; and it will be advisable to move to Geneva, or somewhere in the neighbourhood of that town. There are several spots near Geneva which
are

are reckoned very healthy, and where the climate is good in summer. Mornex is one of these. It is to be recollected that I am speaking of those who have not advanced beyond the first stage of phthisis ; and to whom a journey of a few days will prove rather an amusement than the contrary. Short excursions, such as may be made without much exertion or fatigue, are to be recommended ; and about the commencement of October the patient may return to Italy, or the South of France.

If his winter residence has been at Lisbon, he should certainly quit that city as soon as the hot weather sets in, and remove to a cooler situation, to Cintra for example, which is only about twenty miles distant from the capital, and where the country is eminently beautiful, and the air always cool and refreshing.

I have thus stated concisely under what circumstances it strikes me that
a warm

a warm climate is beneficial to persons who labour under pulmonary affections ; and what, as far as my judgment goes, are the most eligible residences for such persons. I shall be very glad if these remarks should prove in any degree useful. It is my earnest wish that phthisical patients should be sent abroad upon the first appearance of the disease ; or that, if it has become confirmed, they should be spared what I cannot but consider the useless fatigue and anxiety of a long journey, as well as the discomfort of being in a foreign country, at a distance from their families, and deprived of those numberless conveniences which sooth the hours of sickness, and which can be procured at home only.

I shall conclude this paper with a few remarks respecting the effects of a warm climate upon some other diseases. I must, however, premise that my experience of these effects is as yet

yet extremely limited. Over gout a warm climate has seemed to exert a great and decided influence. I knew one striking instance of this. The patient inherited a predisposition to gout, and the disease shewed itself at an early age, attacking successively almost every part of the body, till he was reduced to a state of great suffering. He was almost completely deprived of the power of walking ; and what was more serious, his stomach had become materially deranged, and he had occasionally pains in his head which gave him considerable alarm. During this gentleman's journey from England he had a severe attack of atonic gout, which confined him for several weeks. Late in the autumn he arrived at Nice in a state of much debility, and with great derangement of the stomach and bowels. He there put himself under the care of an English physician ; and partly by medicine, but chiefly by climate, as he thought,

thought, he got considerably better. Indeed, during his stay at Nice, viz. from November to the end of May, he enjoyed far better health than he had done for several years before. The stomach recovered its tone in a great measure, the pains of the head left him, and he recovered the use of his feet so far as to be able to walk a considerable distance without inconvenience. I never met with any one who appeared more fully satisfied of the benefit he had derived from climate than this gentleman.

In obstinate cases of chronic rheumatism, a warm and equable climate has been of service. It has also palliated asthma, but I have not seen enough of its effects in these two diseases to speak with any degree of confidence respecting them.

It has of late been the fashion to send persons affected by deafness to a warm climate. I have seen two or three of these cases, but the hearing
has

has not appeared to me to be in the least improved by climate. In one instance the patient thinks it has prevented the increase of the complaint. It certainly has not diminished it, and the nature of the deafness seemed to be such as to preclude all hope of a cure. It is hereditary, if I may be allowed the term, and the patient also inherits a temperament most highly nervous. In sending deaf persons abroad more discrimination is, I think, necessary ; and some patients might be spared the trouble and expense of a journey, as well as the disappointment which they must ultimately feel at finding they make no progress towards recovery.

At some future time I hope to be able to enlarge upon the influence of climate over these diseases, of which at present I can speak but very superficially.

Should this communication be deemed in any degree deserving of
notice,

notice, I shall be encouraged to arrange what other medical remarks I have already had it in my power to make during my travels, and to collect further information during the remainder of the time I shall continue abroad.

VI. *On the Medicinal Properties of the Solanum Tuberosum, or Potato Plant.* By JOHN LATHAM, M.D. F.R.S. *President of the Royal College of Physicians, &c. &c.*

Read at the COLLEGE, May 7th, 1818.

THIS plant, whose root has been known in this country for about two centuries, as affording the most innocent food for man, has never been sufficiently examined as to the medicinal properties which reside in its other parts. As a species of *Solanum* it might well be conceived to possess qualities analogous to others of that genus of plants, but experiments to ascertain the fact had hitherto been wanting to fix its character as an article of *Materia Medica*. An accident which I witnessed, as occurring to an animal many years ago, ought indeed to have induced me sooner to have made a trial of its narcotic powers upon the human body : and the effects, produced in the following cases, which

which I take the opportunity of laying before the College, will perhaps entitle it to a rank of no mean estimation in the catalogue of medicines.

From about seven pounds of the stalks and leaves of the *Solanum Tuberosum*, Mr. Hume, the well known chemist of Long Acre, prepared for me nearly a pound of extract, with which I commenced my experiments. There was nothing peculiar in the taste of this extract which might disgust a patient; but I nevertheless gave at first only half a grain of it three times a day, which, by gradually increasing the quantity, I soon raised to two grains at a dose.

EXPERIMENT I.

This was a case of cough, attended with mucous expectoration, in which I first ascertained that two grains of the extract might be considered as a fair dose. As much relief was obtained as might have been expected
from

from the extract of Conium under similar circumstances. No distress was felt in the system ; but the patient imagined that his bowels were rather more relaxed than was usual with him.

EXPERIMENT II.

This was a very old case of rheumatism, with distorsion of limbs and articular nodosities, accompanied also with chronic cough, which occasioned at times a most intolerable headache. Whilst taking the smaller doses of the extract, this patient felt something like the tremor and disturbance which will often succeed to doses of Digitalis. I was induced therefore to suspend the use of the Solanum, but in a few days resumed it again by the desire of my patient, who fancied that she had obtained some little relief. I then gradually increased the dose to a grain every six hours, and in a few days to two grains, giving her a caution, should
stupor

stupor of the head or tremor of the limbs come on, that she should discontinue the medicine. The tremor did come on, and she omitted *one* dose of the medicine, and then resumed the plan with considerable advantage, her cough and other very painful affections being much alleviated. I then directed three grains of the extract with one grain of rhubarb, to form it into a pill, to be taken *three* times a day ; and after an interval of three days found her still freer from pain and more comfortable, as she expressed herself, than she had been for several months. I saw her again in a few days, when she informed me that in the night after my last visit, “ the tremor had again distressed her, and that her head felt as if cleft asunder, opening and shutting, that her comforts were consequently very much deranged ; and although she had ceased to take the pills, and had substituted a rhubarb purge, that
she

she was still under much nervous agitation, but that her sensations nevertheless as to pain were very different to what they were before." I ordered her another dose of rhubarb (twenty grains) and desired her to discontinue all other medicines until I saw her again, which I did two days afterwards, when I found her free from all pain, but with a sensation, as of numbness, in the ears, which had produced something like deafness. She was however in other respects well, with the exception of the nodosities and distorsions, which of course remained as heretofore.

EXPERIMENT III.

To a lady between fifty and sixty years of age, who was subject to calculous complaints, I ordered three grains of the extract *three* times a day. In two days she found considerable benefit; the medicine had purged her, but there was one grain of rhubarb in each pill. In two days more a
piece

piece of gravel which had been fixed in the ureter passed into the bladder, which she soon afterwards voided. She said she felt a "tingling all over her, as if she was about to have the nettle rash." She wished to persevere in the use of the medicine, in which I encouraged her, and in a few days considered herself well.

EXPERIMENT IV.

A lady, nearly forty years of age, was desired by me (August 29), to take three grains of extract of Solanum with one of rhubarb three times a day. She has a sort of "tussis ferina" with a soreness or pain below the ribs on the right side, which is probably muscular, as no fulness is observable on very minute examination, and she has been thus indisposed nearly two years. She has seen several Physicians, who have taken blood from her and blistered her, and employed many very powerful and efficient

cient medicines. Steel, and almost every other tonic, mercury and antimony, and such like deobstruents, purgatives and sedatives, &c. &c. were successively employed without any benefit whatever. A week ago I had seen her for the first time, and then ordered pulvis ipecacuanhæ compositus, with a small portion of hydrargyrus submuriatus; but this day (August 29) I found her not at all better. Her hands were now cold, and had her tongue been less white I should not have hesitated to have given her the usual chalybeate draught with myrrh; but I ordered (as I have before stated) the Solanum, and fixed to see her again that day week, when I found that no material alteration had been experienced. The tongue, however, appeared cleaner, and the strong nervous agitations to which she had been all along subject, and which occasionally ended in an hysterical fit, were somewhat subdued. I now ordered
three

three grains of the extract with two grains of antimonial powder three times a day, and a common saline draught with each dose of the pills. At the expiration of a week I paid another visit, when the tongue was much cleaner, and she had more warmth in her extremities, and still less agitation. She looked better and thought herself better, although the cough did not appear to me to be materially diminished. I ordered the medicines to be continued, with a blister to the chest, and a common linctus. The next week I saw her again. Her cough was certainly better, and her pulse, which had usually been small and fluttering, was now more full and free. I now urged her to accompany her sister into the country, but she strongly resisted our importunities, on the plea that she should be out of reach of her medical advisers; but in the course of the week the apothecary informed me that she had been pre-

vailed upon, and had gone, as we had advised, where she could enjoy good air and cheerful society. This Lady was a little relieved, but I attribute nothing exclusively to the Solanum.

EXPERIMENT V.

For a young man afflicted with Chorea S. Viti, I directed three grains of extract of Solanum with one grain of Rhubarb, three times a day. In a week I saw him again, and found that he had taken the pills very irregularly, in consequence of being in a more heavy or comatose state than usual. He did not appear to be better; and as I could not enforce any regularity as to his plan of medicine, I coincided with the wishes of his friends, and sent him to the coast for the benefit of sea-bathing.

EXPERIMENT VI.

A young gentleman, about twenty years of age, had been afflicted with
almost

almost constant cephalalgia for two or three years. Various had been the means employed to procure relief without any sensible advantage: bleeding, cupping, leeches, blisters, purgatives, antimonials, mercurials, &c. had successively been tried, and afforded not the least benefit. I put him also upon several plans without any permanent relief; Conium with hydrargyrus submuriatus, steel with aloetic preparations, were tried. The rectified oil of turpentine was then given in the dose of an ounce and half at once. Some remissions were certainly experienced for an hour or two in the day, and after the turpentine for an entire day, but then the pain recurred as usual. I now directed three grains of extract of Solanum three times a day, having indeed about a fortnight before given him the smaller doses of one grain without any effect. After having tried the Solanum in the increased dose for a week, he
came

came to me in a much improved state, not having had any pain for a longer time than half an hour in the day, and that in a greatly mitigated degree. In a few days, however, the pain returned, and I directed the dose to be gradually increased to six grains. He could not go beyond five grains, for after each dose considerable stupor was produced for about two hours, when the pain returned, although in a less degree. He now was desirous of returning into the country, where I recommended him a plan of decoction of aloes with chalybeate wine.

EXPERIMENT VII.

I considered this person as labouring under Angina Pectoris, either from water in the chest or from possible organic disease of the heart itself. He was about sixty years of age. For several weeks I continued to prescribe for him in various ways, and at last gave him three grains of extract of
Solanum

Solanum with one grain of Rhubarb thrice a day. His pulse certainly became regular, and he thought himself better, as he had been able to call upon me with less difficulty. He felt the same tremors from its use as were observed in other cases.

The foregoing cases are sufficient to shew that the Solanum Tuberosum possesses very considerable narcotic powers. Several other cases I might have detailed, had not the different experiments always given me very similar results ; but the more obvious properties being evident, and the dose with tolerable accuracy having been ascertained, I must leave the more indirect effects to be investigated hereafter by slow and patient observation. A medical friend of mine has already advanced the dose in a case of cancerous uterus to thirty grains at a dose three times a day ; but of course without permanent benefit. I know very well that varied and repeated trials in
a great

a great number of diseases, and under every possible difference of circumstances, ought to be made, before the true value of *Solanum* as a medicine can be properly appreciated; for the experience of ages in many of our best and most active remedies, has even yet been but barely sufficient for the purpose. I am unwilling to say more about the *Solanum Tuberosum*, lest I should hereafter be found to have said too much; but I think it superior to *Hyoscyamus* and *Conium*, and therefore with confidence recommend it to my professional brethren, not only in cases where those medicines have been most commonly employed, but generally in all chronic cases where there may be excess of painful irritation or irregularity of action.

Harley Street,
Dec. 2, 1817.

P. S. Besides the extract above described,

scribed, an extract has also been made from the potato-apple, but which I have not as yet employed. A vinous infusion or tincture has also been prepared, by taking four ounces of the dried leaves of the plant (of which one part was found to be equivalent to five of the fresh) and macerating for ten or fifteen days in two pints of diluted alcohol, in the proportion of *one* part of the rectified spirit to *three* parts of water, both by measure. And this diluted spirit I would recommend to be employed instead of *white wine*; for as the proportions would be fixed and the strength consequently invariable, that uncertainty which always arises from the difference of wines would be obviated, and the tincture would be the same in the shops of every compounder.

VII. *On certain painful Affections of the Intestinal Canal.* By RICHARD POWELL, M.D. *Fellow of the Royal College of Physicians, &c. &c. &c.*

Read at the COLLEGE, May 7th, 1818.

A KNOWLEDGE of the causes by which any given train of symptoms is produced is always of importance, and they are often very obscure. I therefore trust that any attempt to establish the relations between them, may not be wholly undeserving the attention of the College.

Whenever violent pain takes place in the epigastric region of the abdomen, exacerbating in paroxysms accompanied by sickness, yellowness of the eyes and skin, and urine, by clay-coloured fæces, and without any proportionate increase of action in the circulation, biliary concretions are supposed to be forcing their way through the ducts ; and when these symptoms abate, it is inferred that their passage
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into the duodenum has been effected. It is usual also to go a step further, and to direct that the subsequent evacuations be examined, for the purpose of discovering the concretion thus supposed to have passed. It however mostly happens that the search is reported to have been ineffectual, and it is supposed that the concretion has been overlooked, for the evidence of its existence, deduced from the symptoms, has been held to be only a little less complete, but by no means doubtful.

This has so often happened that I have been compelled to ask myself whether my opinion of the existence of concretions, as the cause of the disease, may not have been incorrect? And I have further thought it may have been so, when I have reflected that in the greater number of cases where biliary concretions have been actually found in the gall-bladder after death, they had (as far as my own observation

observation has gone) been productive of no marked previous inconvenience, and had not been suspected to exist during the life-time of the patient.

I wish, however, by no means to be understood as denying the well established fact that such concretions do often pass the ducts, or that the symptoms of their passage are well described and generally known. I myself took up the inquiry into this subject in the *Gulstonian Lectures* of the year 1800, which were afterwards printed. It is my present purpose only to state, that similar symptoms are also referable to another cause, which is presently to be described. Nor even although it may not lead to any marked peculiarity in practice, will the statement I am about to make be wholly without its use. Biliary concretions are most commonly numerous, and the painful passage of one is only the precursor to that of another; the attack is therefore expected

pected to return again and again, and the prospect of a speedy termination of his sufferings cannot be fairly urged to the patient in consolation. But if I am right in my supposition, the sort of attack to be here described is more isolated, and when once it has passed, its recurrence is by no means equally to be dreaded as is that of the former.

Whenever then I have suspected the passage of biliary concretions, I have directed, under my own superintendence, a more minute examination of the evacuations than is usually made by the ordinary attendants upon the sick. The usual large pan has been filled with water, and the fæces stirred in it, after which the water has been left at rest long enough to ascertain whether the concretions, as they often do, have risen to the surface. This first water has then been cautiously poured away, and repeated similar affusions have been made, with
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some allowance of time for subsidence between each, as long as any thing appeared to be dissolved. The residue at the bottom has been examined last.

In the cases to which I refer, this residue has exhibited a large quantity of flakes mostly torn into irregular shapes, and appearing to have formed parts of an extensive adventitious membrane of no great tenacity or firmness. In the first of the cases which came under my notice, this membrane was passed in perfect tubes, some of them full half a yard in length, and certainly sufficient in quantity to have lined the whole intestinal canal. In the others also the aggregate quantity has been very large, and it has continued to come away for many days, but it has been in irregular thin flakes of not more than two inches extent, and not, as far as I could discover, of the perfect tubular form, which would probably
also

also have been broken down by the agitation in water, if it had existed on its first passage out of the body. I have definitely examined four such cases, in all of whom the leading symptoms have been similar, and have led me to suspect the passage of biliary concretions at the time. They have all been adult females, and have occurred in private practice. I had attended but one of these previous to this particular attack, and she had frequently suffered from occasional pain in the intestines and derangement of her powers of digestion, with flatulence and a sense of suffocation. She was always relieved at the time by mild opening medicine, and believed herself able to prevent the attacks of pain from increasing to any serious degree of violence, by repeating it according to circumstances. A similar history of liability to frequent recurrence of pain, accompanied by indigestion, was related to me in the
other

other instances. The more violent seizures under which I saw all the patients, consisted in a sudden and excessive pain in the epigastric region, increasing in paroxysms very frequently, rather relieved by pressure of the patient herself at the time, but leaving great soreness and tenderness during the intervals. This state continued under four days ; during it the stomach was very irritable, and the tongue coated and clammy. Jaundice came on at an early period, and the stools were white, brown, or somewhat greenish, and streaked in colours, until the films began to pass, when they were mixed with a full sufficiency of bile, but not at first of a healthy colour. The pulse throughout was calm, moderate and natural, in none of the instances amounting to 90. It of course did not indicate the existence of any inflammatory action, and still less was it disposed to assume that peculiar irritative quickness

ness which marks enteritis. In one case I noticed a very considerable hardness and contraction of the abdominal muscles, and in another there was superadded to the above symptoms a difficulty in passing the urine, with pain in the region of the bladder, and here the use of the catheter became necessary.

As I suspected the passage of concretions in these cases, I did not deem the abstraction of blood to be proper, neither did I direct the use of emetics*. I gave opium, but found the relief from it was only slight and temporary. The submuriate of mercury in repeated doses was productive of no benefit. The practice, indeed, which appeared to me most advantageous, was the steady use of a mixture of the *Infusum Gentianæ Compositum* and *Infusum Sennæ*, with the addition of

* Such a medicine had been given in one of the cases before I saw it, and had rather increased than diminished the inconvenience.

from nlx. to nlxx. of *Liquor Potassæ*, repeated so as to produce four or more stools in the twenty-four hours. Under its use the flakes first separated, and continued to do so in great abundance; the jaundice disappeared, and the patients recovered health and strength. I might have mentioned that the purgative employed at the time the attack began, in one case was the *Magnesiæ Sulphas* in *Mistura Amygdalarum*, for the purpose of adding that in another also saline purgatives seemed less beneficial than the class of which I have mentioned one. Under their particular operation the evacuations were more frequent and aqueous, but the canal did not appear so completely to discharge its contents, and the flakes were fewer in quantity. This observation, however, only serves to confirm the well established difference between those cathartics which produce copious aqueous evacuations, and those which rather excite

excite the action of the intestines to the dislodgement of their contents.

The formation of adventitious membrane has not been so frequently observed in the intestinal canal as it has in circumscribed cavities ; and I know not that any description of the symptoms accompanying such a state has heretofore been given. The appearance which comes nearest to it, both in resemblance and situation, is the membrane formed in the trachea under croup, but the symptoms are there more violent and destructive from locality of situation.

The most remarkable circumstance in these histories, is the production of an effect usually ascribed to inflammatory action without its previous existence. Coagulable lymph is described as occasionally found thrown out upon the villous coat of the intestines after inflammation, but here under an acute attack such a state was neither indicated by the sort of

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pain,

pain, which might rather be called spasmodic, nor by the state of the pulse and skin, nor by the event, for I take it that positive inflammation would not have terminated so favourably in four different instances under the treatment employed.

I missed the opportunities of examining the qualities of the flakes thus evacuated, but a quantity of them was put into spirit of wine for preservation, and was contracted by its action, so as to look almost fibrous rather than membranous, as at first, by which one is led to suppose that they may consist of albumen, though this is only a supposition ; and as it differs so much from the usual characters of increased secretions from other membranes of the same class, it is perhaps fair to suggest that it may be poured out from the exhalants rather than from the mucous glands of the intestines. At any rate it is very different from that effusion which often coats

coats the serous membranes of circumscribed cavities, which remains attached to the part from which it is effused, permanently agglutinates neighbouring surfaces, and becomes an organized substance.

VIII. *Narrative of Facts relative to the repeated Appearance, Propagation and Extinction of Plague, among the British Troops employed in the Conquest and Occupation of Egypt, in the years 1801, 1802, and 1803 ; with Remarks on its Contagious Nature ; being the result of Observations personally made by JOHN WEBB, Director-General of the Ordnance Medical Department. Communicated by DR. HUE, Registrar.*

Read at the COLLEGE, May 18th, 1819.

Woolwich, 31st March, 1818,

THE President and Committee of the Royal College of Physicians having requested me to acquaint them with the result of my own personal observations on the Plague, and more particularly with reference to its contagious nature, I have the honour to lay before them the following narrative of facts I personally observed at the close of my services in Egypt, relative to the nature and effects of pestilential contagion ; to which I have prefixed a cursory view of the repeated appearance of plague in different corps

corps of the British army during the campaign of 1801, and the subsequent occupation of Alexandria and Rosetta by British troops ; as I consider a slight sketch of those occurrences necessary, to render the facts which are the more immediate object of this narrative as distinct and accurate as their nature and importance seem to me to require.

After the army had forced a landing in Egypt, and had driven the French under the walls of Alexandria, the soldiers who had been wounded in the actions of the 8th, 13th and 21st of March, (about 2000) were collected at Aboukir, and accommodation was provided for them, partly in the huts the French had occupied, and partly in temporary hospitals erected by the carpenters of the fleet. These men had been but a short time under treatment when the Plague appeared among them ; and although constant vigilance was exercised, and
every

every effort made, whenever a case occurred, to prevent the extension of the disease, it carried off many of the patients who had been debilitated by their previous sufferings, and a still greater number of the servants who attended them.

As the losses the French had sustained in the field had rendered them incapable of quitting their intrenchments, the British General in Chief resolved to blockade Alexandria, and to open a communication with the Delta ; in pursuance of this determination, he detached a body of the troops on the 2d of April to proceed against Rosetta, of which I had the professional charge.

When Rosetta had been taken, a spacious building was allotted for an hospital, and such of the soldiers as had fallen sick during the short time we had been separated from the body of the army were admitted into it. A few days after this hospital had been
established,

established, I found, in making my usual morning inspection, that a large Anthrax had appeared on the right cheek of one of the patients in the course of the preceding night, and that both the parotid glands were so enlarged as to threaten suffocation. The unfortunate man was immediately removed, with his clothing, to the Lazaretto belonging to the Port; and the ward he had lain in was evacuated, and his bedding and bedstead were burnt. He died; but the other patients escaped the disease.

Early in May the division of the army in the vicinity of Rosetta had been sufficiently reinforced to advance against the fort of Rhamanieh, which was taken on the 9th of that month. From this period, until the close of the operations on the banks of the Nile, which terminated in the surrender of Cairo on the 28th of June, the return of the troops to Rosetta at the end of July, and their rejunction

tion with the army blockading Alexandria about the 12th of August, they suffered with progressive severity from dysentery and ophthalmia, but very little from pestilential contagion. Cases however did from time to time occur, which I personally ascertained to be Plague, in one of its most deadly shapes ; and although they were few in number, they were still sufficient to convince me that the comparative freedom of the troops from that additional scourge, was chiefly owing to the nature of the operations they were employed in ; which, whilst they created no necessity for long and fatiguing marches, rendered movements of the different corps to fresh positions requisite, generally at short intervals ; whereby the men were successively marched from camps, where causes of disease had been introduced, or had begun to accumulate around them.

On our arrival before Alexandria (at which time the blockade of that fortress

fortress was converted into a vigorous siege,) I learned from the superior medical officers there, that whilst the stationary corps had scarcely been more fortunate with regard to ophthalmia and dysentery than their brethren who acted against Cairo, they had been less so with respect to the Plague ; for so soon as the communication had been opened with Rosetta, crowds of Arabs had flocked from the Delta to the camp with provisions, for which they obtained a ready sale ; and although a market had been established for them at a distance from the camp, and regulations had been adopted to prevent unnecessary intercourse with the soldiers (which were still enforced by the Provost Marshal) they were found insufficient to guard against the communication of disease. Attacks of Plague were not therefore unfrequent in the camp ; but as every man detected with the slightest febrile complaint

plaint was instantly separated from his comrades ; and as every case in which malignant symptoms appeared was conveyed to the Pest Hospital at Aboukir, the contagion never so extended itself in any corps as to create alarm among the military ; but the suddenness of its effects, which sometimes manifested themselves where they were least expected, was a constant source of solicitude and watchfulness to every class of the medical officers.

Such were the circumstances under which the British army completed the conquest of Egypt, on the 1st of September, 1801, by the reduction of Alexandria. This important event having rendered the greater part of the forces disposable, the attention of the General in Chief, and the efforts of his staff officers, especially those of the medical department, were directed to the employment of every practicable means to preserve the regiments

giments about to depart from carrying contagion along with them, which was happily effected, and all the troops had embarked, and had sailed for Malta by the end of October, except the battalions ordered to garrison Alexandria, and the Indian army, under Sir David Baird, which had just arrived at Rosetta, and was to await its orders from England.

On the 5th of November, General Lord Hutchinson, prior to his delivering over the command to a junior officer and embarking for England, committed the care of preserving Alexandria and its garrison from infection to a Board of Health, composed of experienced military and medical officers, upon whom His Excellency conferred full powers to watch over the state of the army, the police of the city, and the business of the port. They were empowered also to form a code of regulations for the performance of quarantine, according to the rules

rules and under the penalties established by the British Legislature ; and to appoint the different classes of persons who were to enforce the same, under their orders. The Lazaretto was confided to the able direction of Doctor Buchan, aided towards the close of the service by the late Doctor Price.

The Board of Health, thus constituted, assumed its authority without delay, and continued thenceforward to exercise its powers with energy and success. But as its regulations, which scarcely underwent any change in the course of the two Plague seasons the British encountered before they were withdrawn from Egypt, communicated the uniformity of their own operation to the routine of the medical duty ; and as an account of the diseases that prevailed in the Indian army during the same period has already been published by an author of high respectability and accurate observation, I shall dwell no longer

longer on this part of the subject, than to state that the honorary appointment of President to the Board of Health was held by Colonel (now Field-Marshal Lord) Beresford, and that of Medical Member successively by Doctor Shapter, Sir James M'Grigor, and myself. My portion of the duty was from the 3d of June, 1802, when the Indian army commenced its march to Suez, until the close of the Egyptian service.

On the 4th of June, 1802, a report was received by the Board that the Plague had broken out at Demanhour and Birket, and that several of the 26th Light Dragoons, who were quartered in those villages, had caught the disease. Some delay in applying the means of disinfection was unavoidably occasioned by the distance of the station from the board, and fifteen persons were attacked before the arrangements for that purpose could be completed; but in a few days

days after these had been employed fresh cases no longer appeared. The corps was then marched to the vicinity of Alexandria, where it performed the customary quarantine.

While the attention of the Board was directed to the state of the dragoons at Birket, they received an official intimation from the General in command, through their President, that an order might soon be expected from home for the evacuation of Egypt; they were therefore called upon to suggest such measures as they deemed best adapted to enable the General to keep the troops constantly in readiness for embarkation. In pursuance of this request, the Board recommended to his lordship to order the commanding officers of regiments and corps to make an accurate inspection of the necessaries belonging to the soldiers, and other persons under their authority; to burn all superfluous articles, and have the
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rest thoroughly washed ; to encamp the different battalions separate from each other ; and to have steady non-commissioned officers appointed by each, to obtain their supplies. These precautions, which were immediately adopted by his lordship, separated the healthy part of the army from many sources of disease. Similar measures would also have been employed for the removal of the sick, but the hospital marquees were unfortunately too much worn to protect them from the weather. The Board had therefore no alternative but to keep the men in their respective regimental hospitals, which had been established in Mosques, and to cut off all communication between them and the inhabitants of the city, as far as strict orders, when necessarily enforced by sentries who neither knew nor thought of the dangers which surrounded them, could effect that object. Thus, the several corps of the army were placed, both

with regard to the city and to each other, in a species of quarantine, in which they afterwards continued, notwithstanding the period of their departure was delayed much beyond what was originally expected.

From the beginning of July to the end of October no case of malignant fever appeared (to my knowledge); either in the city or among the troops. On the 12th of November the Board was hastily summoned, in consequence of the Plague having broken out in an Okal, occupied by eight Jewish families, consisting in the whole of thirty-six persons. A minute inquiry was immediately instituted into all the particulars connected with their state, but nothing was discovered which could lead to a well-founded opinion, whether they had brought the contagion with them, or had been allotted infected apartments in this uncomfortable substitute for an inn; but it was ascertained that four of
their

their number had perished, before they made known the existence of the disease.

The alarm created by this occurrence was greatly increased during the nine following days, by several cases of Plague being detected in various parts of the town, principally among the Jews. Under these arduous circumstances, the Board's regulations were promptly and rigidly enforced, and whole families with their effects were conveyed to the observation tents, within the boundary of the Lazaretto ; which measures, although opposed and evaded in every possible way, produced the following result : from the 21st to the 30th of November one person only was reported ; in the month of December, nine ; in January, two ; and to the 13th of February, the date of the last attack, two.

While the inhabitants of Alexandria were in the perilous condition I have described, a ship from Constantinople,

tinople, with troops and pilgrims on board, entered the port. As the Turkish authorities submitted to the British regulations for the preservation of health, this vessel, which had been forty-six days on her passage, was examined by the inspecting medical officer, who reported that the Plague had broken out among the passengers at the beginning of their voyage, many of whom had died, but the exact number could not be ascertained. By landing the people in the quarantine ground, and subjecting them and the ship to the usual means of disinfection, the progress of the disease was speedily arrested.

The state of the population of Alexandria, in the early part of November, naturally directed the anxious attention of Major-General Sir John Stuart (who had succeeded Lord Cavan in the command), and of the Board of Health, to the troops encamped in its vicinity, or quartered
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in temporary barracks, that had been fitted up latterly for some of them, between its ancient and modern walls. Every regiment had a few individuals employed in the city, either in attendance on the general staff, or the magistracy, who suffered in common with the other inhabitants ; but the bodies of eight of the nine corps which formed the army remained free from contagion, and their respective hospitals were scarcely less fortunate.

The Chasseurs Britanniques, the unfortunate exception to the foregoing remark, had been newly raised at Trieste, from the remains of the army of the late Prince de Condé ; and the regiment had not yet been embodied a sufficient time to acquire the degree of discipline to which it afterwards attained ; a state at all times unfavourable to the preservation of the health of troops, but more especially so during the prevalence of contagious epidemics. In addition to this, the
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men had scarcely been a year in Egypt, consequently were influenced in a higher degree by the predisposing causes to disease existing in and peculiar to the country, than the soldiers of the other regiments who were more accustomed to the climate. It happened also that the Mosque, which was allotted for their hospital (on account of its being the nearest to the battalion) was one of the most exposed to communication with the inhabitants.

To the 20th of November, inclusive, the surgeon of the regiment reported the men free from contagion ; but on the morning of the 21st, I discovered, in visiting his hospital, three decided cases of Plague. As the patients lay in a state of stupor, without uttering any complaint, and even answering inquiries with reluctance, the surgeon, who had never before seen the disease, could scarcely be convinced of its nature. The de-
cease,

cease, however, of two of these men on the 22d, and of the third on the following day, the whole covered with vibices, put the matter out of doubt.

The number of this corps affected by the contagion was twenty-six, of whom eighteen were taken ill between the 21st and 28th of November: the others were reported singly, and at intervals of a few days, until the 14th of December, when the disease disappeared. The amount of all other descriptions of persons belonging to the army, who were attacked with Plague in the course of the season, was two officers, and twenty-three non-commissioned officers and privates. Three of these cases require to be briefly noticed.

Lieutenant ———, of the 10th Foot, who had lived a considerable time in Alexandria, in a house of which himself, a young female Arab, and a man servant, John Lee, a soldier in the same regiment, were the
only

only inhabitants, complained, on the 13th December, 1802, of rigor, and other symptoms of fever, which increased until the 20th, when the appearance of a large inguinal bubo decided the nature of the disease. From the first attack Lieutenant —— had been placed in observation ; and now an order was issued to the inspecting medical officer to have him conveyed within the boundary of the quarantine ; but his removal could not be effected until the 22d, owing to the extreme wetness of the weather. A hole having been torn accidentally in Lieutenant ——'s mosquito curtain, John Lee took it (without his master's knowledge) to the 10th Regimental Hospital, in the evening of the 21st or early in the morning of the 22d, and having prevailed on the sentry to admit him, in violation of his orders, he caused one of the patients, named William Bowen, to repair the curtain, which he then carried

ried home ; and accompanied his master into the Pest Hospital, at his own request, where he attended him until his recovery was complete.

Private William Bowen, 10th Foot. This irregularity remained unknown to the regimental medical officers until the 5th of the following month, (the fifteenth, or at least the fourteenth day after its commission), when the surgeon found Bowen had been attacked with fever during the night, and was then labouring under symptoms of a very suspicious nature. As Bowen had been confined to the hospital from the latter end of November, the surgeon was at a loss to imagine how he could have been exposed to contagion, and questioned him very minutely as to his conduct, when the man recollected the circumstances I have stated, and informed him of them. The case was declared Plague on the 7th, and he died the same evening.

Captain

Captain ——, of the Royal Artillery, a gentleman of amiable disposition and retired habits, had often expressed great apprehensions of catching the Plague, and always took the greatest care to preserve himself from infection. He was attacked with fever on the 12th February, 1803. An anthrax and bubo appeared the following day, and he died on the 15th. None of his friends could form the slightest conjecture as to the source of the disease.

Towards the end of February the long expected order for the evacuation of Egypt arrived, and measures were immediately taken to prepare the troops for their departure; among which, the inspection of the mens' necessaries, and the ablutions recommended by the Board of Health in June, 1802, were repeated with the greatest care, under the superintendence of

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the commanding officers of regiments. The convalescents of each battalion were sent on board one of the transports allotted for their own corps, in charge of an assistant-surgeon, with a view to remove them from further risk of exposure to contagion ; and the men in health were directed, at the suggestion of the Board, to embark in linen dresses, and to carry with them their woollen clothing, perfectly clean, for use when afloat. As the several regiments arrived at the beach, every person's effects were again examined, and all articles procured or retained, contrary to orders, were collected and burnt. Thus the embarkation was completed to the entire satisfaction of the General in Chief, who publicly expressed his approbation of the manner in which it had been conducted ; and the Board of Health looked forward to an early and successful termination of their labours ; but they were soon to learn, that even the strictest precautions,

precautions, enforced with unceasing solicitude, may be frustrated in some measure by causes which human foresight cannot prevent.

On the 8th of March a report was received by the Board from Mr. Millet, surgeon of De Watteville's, that Corporal Jean Jeanpierre, of that regiment, had been attacked in the Janus transport with malignant fever. This case having been examined by the inspecting medical officer, and declared Plague, the vexatious intelligence was instantly conveyed to Sir John Stuart, with a suggestion that the troops on board the Janus should be relanded with the least possible delay, and encamped near Fort Marabout. In making this communication, I offered my services to remain in charge of the detachment, which the general cordially accepted. I then directed Mr. Millet to procure and report to me the best information he might be able to obtain, as to the probable

probable source of the contagion that had appeared in the Janus, from whom I received the following reply on the 11th, sanctioned by the signature of his commanding officer.

“ De Marabout, ce 11 Mars, 1803.

“ MONSIEUR,

“ Le convalescents et quelques malades du regiment de Watteville, formant ensemble le nombre de 14 hommes, furent embarqués à bord du transport le Janus, le 22 Février, sous la direction de l'assistant chirurgien Boidin. Le 1^{er} Mars, il fut requis par le capitaine de visiter un de ses matelots malades. Cet homme avoit été ivre la veille. Mr. Boidin lui administra un emetique. Le lendemain, 2, il passa assez bien la journée, et la nuit suivante, il se plaignoit seulement d'une grande altération : il fut maintenant à la diète, et à l'usage de la limonade. Mr. Boidin quittant le Janus ce jour, pour aller à sa nouvelle

velle destination, en me rendant conte des malades qui y restoient, me parla aussi de celui-là. Je le vis le 4, au matin : il étoit dans le travail d'un nouvel émétique que le capitaine lui avoit administré. Je trouvai ce malade dans un état tres dangereux. Je en rendis conte à Mr. Le Lieutenant-Colonel de Watteville, commandant de la troupe ; et j'invitai le capitaine à appeller un officier médical plus compétant que moi pour juger ce cas ; en enonçant neantmoins mon opinion sur l'état du malade, sur lequel je n'avois remarqué aucuns signes de peste. Mr. Blackwell, appelé pour visiter le malade, vint quelques heures après moi : vit le cadavre, et declara qu'il n'étoit point mort de peste ; non-obstant cela on fit dans le réduit où étoit mort ce mâtelot, comme dans toutes les autres parties du bâtiment habitées par la troupe, les fumigations et les lavages convenables.

L. de Watteville,
Lieutenant-Colonel.

(Signed) "MILLET,
"Chirurgien du
"Regiment de Watteville."

The report, alluded to in the foregoing letter, was made to the Board of Health at the time mentioned by Mr. Millet, which led them to conclude the complaint that had proved fatal to the sailor was not contagious. It ought also to be remarked, that the inspecting medical officer saw the body immediately after the man's decease, which naturally tended to mislead him in forming his opinion.

On further inquiry into this case, I unexpectedly discovered that the day after the sailor had expired another soldier of the detachment, André Bukovitzsky, died suddenly, after an apparently slight indisposition of twenty-four hours. Surgeon Millet, who had scarcely ever seen a case of Plague, attributed this man's death to "violent exertions in vomiting a quantity of food he had taken during the day." He did not therefore report it according to orders, and the Board of Health remained ignorant of the occurrence.

Corporal

Corporal Jeanpierre, who was reported on the 8th, as already stated, died before he could be removed from the ship. On the 10th the detachment was landed, and encamped near Fort Marabout, under the command of Lieutenant-Colonel De Watteville. The equipments of the Pest Hospital, and the arrangements for our subsequent supplies having been completed, I left Alexandria for the encampment on the morning of the 11th, but not until I had learned from Colonel Beresford the unwelcome intelligence that Meinhart Friener, another soldier of De Watteville's Regiment, had been attacked with Plague on board the Major transport, and that that vessel had been ordered to follow the Janus to her anchorage. On the 12th the fleet weighed, and proceeded on their voyage to Malta.

The introduction of contagion into the Major was not traced so accurately as that into the Janus. The following

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ing is a copy of a memorandum on the subject, I received from Surgeon Millet.

“ Pierre Brünner, entré à l’Hôpital le 1^{er} Mars, pour une dissenterie avec fièvre et tenesme, qu’il avoit depuis plusieurs jours, étant de garde à la coupure du canal d’Alexandrie. Il fut porté le trois à bord du transport le Major, où il est mort le lendemain, 4 Mars, 1803.” His reason for having embarked a man in such a condition, along with healthy troops, was not assigned.

The decease of Brünner was reported by the surgeon immediately to the inspecting medical officer ; but as he at the same time specified the complaint of which the man had died, and that he entertained no suspicion of its being infectious, the report was deemed satisfactory, and no further inquiry took place.

Not feeling satisfied with the defective information I had received of this

case, I availed myself of the ample opportunities my situation now afforded, to make further inquiry into it, both from the officers in the camp and on board ; and the result was, the removal of almost every doubt from my mind as to the nature of the disease, which I believe to have been Plague, modified by the local circumstances the man had been placed in for some time prior to his attack ; but whether the contagion originated with him, or Trienner, the particular state of the vessel at the time it was introduced, increased both its power and sphere of action with alarming rapidity. The tonnage of the transports having been found rather above what was wanted for the troops, the Major was partly laden with corn for Malta, on account of government ; but sufficient space was reserved for a company of infantry and their baggage. After the men had been embarked in her, the weather became extremely wet, which induced

induced the master to close the hatchways of the vessel, for the preservation of the corn. Thus the circulation of air in the 'tween decks was impeded, and the fomites acquired thereby a degree of destructive virulence, never before experienced in the British army*.

In the course of the morning of the 12th, and the morning of the 13th, Captain Fischer's Company, with all persons belonging to it, were landed from the Major, and exhibited an affecting presage of the loss they afterwards sustained. As I approached the beach to examine them, the first object that presented itself was a

* Mr. Hodson, the medical officer who had charge of the Plague patients, reported to me on the 19th of March, "the cases he had received were so desperate that medicine produced little, if any, effect upon them." This gentleman was well qualified to judge of their nature, as he had served in the Pest Hospital near Alexandria, under Doctor Buchan, from the beginning of the season.

young woman supported in a chair, (Francisca Kennis), moaning under oppressive disease. She stared wildly about, quite insensible to every object around her, and there was a muddy glistening in her eyes, which I had seen described, but had never before observed. Her husband stood over her in the deepest distress, and held a lovely infant to her breast, who tranquilly sucked the poison that soon afterwards destroyed him. I feared, at first, that force would have been necessary to separate the father from his wife and child, but he at length yielded to entreaty, and was removed from the infection, though too late to save his life. She was conveyed to the Pest Hospital, where she soon expired; and the child was confided to an Arab, who fed and watched over it with the greatest care. Six others, evidently with Plague, were discovered during the remainder of the examination, and three more with fever of a

very suspicious character, who were placed in observation.

On the 28th of March, the fifteenth day after the separation mentioned in the preceding paragraph took place, the infant was attacked with Plague, and languished until the 4th of April, when death terminated his sufferings.

The ground we occupied was well adapted in some respects for separating the people from each other, and for disinfecting their baggage, which process was commenced without a moment's delay. The nearness also of the encampment to an excellent beach, afforded every facility for the men, divided as they were into different classes, to bathe at regulated periods, under the superintendence of their respective commanding officers. The strictest orders were likewise issued to the masters of the transports for the purification of the vessels and their crews.

The subsequent occurrences of this distressing scene followed each other
with

with such rapidity, that I find it impossible to attempt an intelligible description of them. I shall, however, transcribe a few passages from my official communications during the period, which will convey some idea of the most prominent facts.

Extracts of Letters addressed to Major Missett, British Resident, acting as Consul General in Egypt.

“ Camp near Marabout,

“ DEAR SIR,

20th March, 1803.

“ I inclose for your information, a Return of Sick belonging to the Detachment encamped at this place, and to the Janus and Major transports, with the casualties that have occurred since I commenced this duty. The first glance of it will shew how rapidly the disease has been communicated, and the number of deaths but too plainly evince the virulence of the contagion.

“ Captain Fischer’s Detachment
has

has sent more than half its number to the Pest Hospital, but I trust the fury of the infection is nearly exhausted in that quarter, as during the last two days no fresh case has been reported.

“ Lieutenant-Colonel De Watteville’s Division continued healthy until the 15th, and our hopes had become sanguine that it would have entirely escaped ; but on that day three suspicious cases appeared, and the disease has since continued to manifest itself with great malignity ; three persons have been taken ill, and separated this morning, two of whom have certainly symptoms of Plague.

“ The crew of the Janus has been three days without complaint ; this circumstance, and the perfect cleanliness of the ship, lead me to hope that the malady is arrested on board her ; but I am conscious this hope rests, for the present at least, on a very slender foundation.

“ The

“ The ship Major is much more unfortunate. We received two persons from her on the 15th, one of whom had a pestilential sore, and a bubo, but the other, who is the first mate, had only a slight complaint, greatly magnified by his fears. This morning three of the sailors have been landed, all decided cases of Plague ; and the symptoms so malignant, that I have the greatest apprehensions for the remainder of the crew.”

“ *Marabout, 22d March.*

“ Captain Fischer’s Detachment, and the Janus transport continue healthy. As we have received only two men from Colonel De Watteville’s Division since the 20th, I hope it will soon be in my power to report it also free from disease. The last persons taken ill were from the same marquee, which makes me very anxious to separate them from each other ; but this will be impossible unless you
can

can procure me the additional tents I requested the commissary to apply for.

“ The state of the Major transport is so desperate, that I must beg your instructions respecting her. In my last I mentioned our just having received three of the sailors with Plague, two of whom were attacked so violently that they died in about eighteen hours after they had been landed. The third is better, and we entertain hopes of his recovery.

“ As I felt convinced that the only mode of saving even a part of the crew, was to disembark as many of the men as could possibly be spared, without risking the loss of the vessel ; I proposed to the master (on the 20th) to moor the ship, and to land half of his people, which he did immediately ; and now the hands relieve each other daily. By this arrangement no man is allowed to sleep on board who is not actually on duty.

“ But

“ But even this measure has not, as yet, produced the wished for effect. The carpenter complained yesterday evening of being feverish, and to-day he has an anthrax and a bubo. Thus of seventeen persons composing the ship's company, two are dead of the Plague, one of whom was the second mate, and three are now in the hospital with the same disease. The first mate is also on shore, but so alarmed that he is incapable of rendering the least assistance to the master.

“ The ship has been repeatedly cleansed, fumigated, and white-washed. She has a hundred tons of corn aboard, and about eight tons of beans. The former, part in bulk and part in sacks, but stowed in one of the holds, with the hatches battened over, and made water-tight. The beans are in the fore-hatch, and packed in baskets, so that there are no parts of the cargo susceptible of contagion but the sacks and the thread used in closing the baskets.

baskets. I think, however, the beans ought to be disembarked, as some of the sailors slept in the place where they were stowed, and one of them is now ill of the Plague."

(To Doctor Shapter, Inspector of Hospitals.)

29th March.

"It is astonishing how the officers, especially of Captain Fischer's Detachment, have escaped. Of six servants, four are dead of the Plague, and yet their masters continue perfectly well, and, I have every reason to believe, will continue so.

"As I have had but few opportunities of observing the nature and progress of this dreadful disease, I have no means of making a comparison between the effects of the present virulent contagion, which has carried off so many people, and that which appeared from time to time in the British army while they were in this country; but Mr. Hodson declares, that

that he had no conception of its powers until he did duty here ; to use his own words, ‘ ’till now I have never seen the Plague.’ ”

Towards the end of March the occurrence of cases became less and less frequent, and the gloom that had reigned in the camp gradually dissipated, until the officers and men, who had escaped the disease, recovered their wonted cheerfulness. On the 31st I informed Major Missett that no attack of a suspicious nature had occurred after the 27th, and as I hoped I might consider our quarantine begun from that date, I requested his attention to the expediency of taking immediate steps to have the ships prepared for sea, and the troops supplied with new blankets, and various articles of clothing, in lieu of those that had been destroyed. I also recommended that a convenient building might

might be procured in Alexandria, or its vicinity, for the reception of the convalescents from Plague ; who were to be left in charge of Mr. Hodson, under the protection of Mr. Briggs, the pro-consul. These suggestions were acted upon immediately.

But although our prospects had begun to brighten, our subsequent progress towards a fit state for departure was not without interruption. As I was dispatching my letter to Major Missett, the master of the Major transport came to me with one of his sailors, who had an inguinal bubo, but was otherwise in perfect health. The man declared he had had it three days, and attributed it to cold. I was however satisfied, after a careful inquiry into his state, and an examination of his leg and thigh of the same side, that it was an effect of pestilential contagion, but in its mildest form. He was therefore placed in a separate tent, and a gentle aperient was

was administered, which was all the medicines he required. On the 2d of April I found the swelling had begun to diminish, which it continued to do, until it entirely disappeared.

On the 5th of April, our feelings were much excited by one of Lieutenant-Colonel Watteville's Detachment being attacked with fever. This melancholy case was attended with circumstances worthy of particular remark. Serjeant Sharpij had been twenty-six days on shore, and had enjoyed, as a non-commissioned officer, superior advantages of accommodation to what could be obtained for the private soldiers. On the 4th of April he was apparently in perfect health, and took a walk along the beach, when he was tempted to collect some shell-fish, which he cooked, and ate on his return to camp. Soon afterwards he was seized with violent vomiting, which continued several hours ; then symptoms of fever appeared,

peared, and his case was reported to me of a doubtful nature. I saw him in this state on the morning of the 5th. He seemed extremely anxious to conceal the disease he was labouring under, and attempted to stand up whilst he spoke to me, but he staggered and fell. I ordered him immediately into observation ; and on the following day I had the mortification to find that he had a large bubo in each groin. He survived this attack, and was sent to Alexandria with the other convalescents ; but I learned from Mr. Hodson some time after I arrived at Malta, that one of his buboes sloughed so deeply into the muscles of the thigh that it destroyed a branch of the profunda femoris, and he sunk under the consequent hemorrhage.

The attack of Serjeant Sharpij was the last that occurred of a malignant nature. Our quarantine recommenced therefore at the date of his separation from

from the detachment; and notwithstanding we were repeatedly alarmed during its progress, by individuals being taken ill of complaints that proved in the issue to be of no importance, the period of twenty-one days was completed without further interruption. As it approached its close, the convalescents, nineteen in number, were conveyed to Alexandria, under the care of Mr. Hodson; and were lodged in a comfortable hospital that had been prepared for them, under the direction of Mr. Briggs.

When our Pest Establishment at camp was broken up, I discovered that the Arab servants who had been employed in it, had secreted a great part of the clothing of the men who had died of the Plague, some of which they wore with great satisfaction, and perfect impunity.

I wrote on the 25th to Mr. Briggs, warning him of this discovery, for the information of the Padrona Bey, and

and recommending that measures might be adopted to prevent the introduction of the infected articles into the city. The Bey replied, " they may bring in any thing they please*."

The Major having again been white-washed throughout, and both transports being in readiness for sea on the 27th of April, the detachments embarked on board their respective vessels in the course of the 28th, and we sailed the following morning. We arrived at Malta all well on the 24th of May, where, after a further quarantine of forty days, we were admitted to pratique on the 3d of July.

* Prior to my departure from Marabout, I requested Mr. Briggs to favour me with an account of any consequences that might arise from this decision, who kindly corresponded with me on the subject, until the end of the following year. I learned from him, with great surprise, that the city continued even more free from Plague during the whole period, than it had been for several years before its occupation by the British.

I fear this narrative will be found to bear evident marks of my having sketched it with a mind occupied by subjects of a different nature, which form the greater part of my present duties ; but conscious as I am of defects in this and in other respects, I pledge myself for the correctness of the statements it contains, which I have faithfully extracted from official documents, and from a journal kept at the time, with all the care I could bestow upon it.

As the inquiry of the President and Committee refers chiefly to the contagious nature of Plague, which I trust I have established on the most conclusive evidence ; and as I have extended the limits of this paper much beyond what I at first intended, I shall abstain from enlarging on the facts it exhibits as to the action of pestilential contagion, the classes of persons most obnoxious to its influence, and the variety in the periods
that

that elapsed between its communication and the appearance of the disease. This I do the more willingly, as I believe the circumstances of each are sufficiently detailed to enable the learned body by whom they are to be considered, (with the aid of the annexed General Return), to form their own conclusions upon them.

The question, whether Plague ever arises spontaneously or not, is, I conceive, involved in much obscurity, and must continue so with respect to Egypt, as long as almost every house inhabited by a Mahometan, and more particularly by a Jew, is made a nidus of contagion, and the people and their government continue, not only to view its ravages with the greatest indifference, but to consider every means of preventing its dissemination a vexatious and useless restraint. It is generally believed by the most intelligent of the inhabitants of Alexandria, Rosetta and Cairo, that the Plague

is sometimes produced by a concurrence of local causes, more especially in the districts fertilized by the overflowing of the Nile; from which a most offensive miasma ascends after the river has deposited its slime and retired within its banks*. Remittent fever is certainly endemic in those districts at that period of the year; but I was unable to discover whether any accurate distinction had been made between its symptoms and those produced by pestilential contagion.

So far as my own experience, aided by inquiries made in October, 1801, (when I again visited Rosetta and Cairo) enable me to form an opinion

* Captain Hayes, of the Royal Engineers, a young officer of great promise, who had been left in Egypt by the late Lieutenant-General Sir John Stuart, on a particular service, was violently attacked with bilious remittent fever in July, 1803, just as he had completed a journey across the Delta from Damietta to Rosetta. He died at the latter place on the 26th of that month, after a very short illness.

on this intricate subject, I am inclined to consider the Plague as much an inhabitant of Egypt as the small-pox is of England*. It is well known that the Egyptians denominate the winter, and the early part of spring, their "Plague Season," which they acknowledge never passes without the disease appearing in some degree. The difference therefore between a Plague year, and one of comparative freedom from it, is this: in the one, thousands are swept away by its destructive power; in the other, cases occur but seldom, which are thought casual and of no importance; and

* A fatal experiment, to try whether pestilential contagion existed in a palpable form, was made at El Hammed, near Rosetta, in the year 1802. Doctor Whyte, (an hospital assistant) inoculated himself twice with bubonic matter, on the 2d of January, by friction, and on the 3d by incision. He was attacked with rigor, and other symptoms of fever, on the evening of the 6th, which proved to be Plague. He became delirious on the 8th, and continued in that state until the evening of the 9th, when he expired.

the

the few of these that terminate fatally are scarcely noticed.

It ought also to be remarked, that the rise, progress and abatement of Plague in different years, bears a striking resemblance to each other. At its first appearance, which is usually in November, it assumes its most deadly form ; and those affected by it sink into the grave almost without complaint. During the winter, and the beginning of spring, it scarcely manifests a diminution of its virulence; but towards the end of spring, when the weather increases in warmth as the summer approaches, its attacks become less frequent, and its malignant symptoms subside into the appearances of ordinary disease, still however retaining the characteristic one of glandular affection. Towards the end of June it is said to disappear ; but I entertain some doubts of the accuracy of this statement*, not-

* When I was at Rosetta, in October, 1801,
Lower

withstanding the period of its supposed cessation is announced to the public annually by the Turkish government on the 24th of June, by a discharge of cannon.

Various other particulars occur to my mind, in confirmation of what I have advanced, but presuming those I have already adduced are amply sufficient to prove the contagious nature of Plague, I shall only beg to say, in conclusion, that if I were again so circumstanced as to have the care of preserving an army, or a community, from the calamities it occasions, con-

Lower Egypt was declared free from Plague ; yet in conversing on the subject with an English medical gentleman, who was on duty there, he assured me that he had seen a marked case of Plague that very morning, though divested of its malignant character. After he had ascertained the state of the inguinal glands, he declared the nature of the disease ; but the patient's friends told him it was impossible, for " the Plague was never known in the country at that period of the year."

fided to me, I should consider no plan of precaution of any avail, if not founded on a full admission that pestilential contagion is one of the most subtle poisons that exist, as well as the most destructive to human life ; and that there is no safety for strangers in the countries subjected to its ravages, whether the disease be prevalent or not, but in adopting and uniformly observing the strictest precautions to avoid the sphere of its action.

No. 1.

RETURN of persons belonging to De Watteville's Regiment, and to the Janus and Major Transports, attacked with Plague, from the 11th to the 27th March, 1803, (both days inclusive.)

<p> DATES OF ATTACK. </p>		<p> Lieut. Colonel De Watteville's Detachment. </p>		<p> Janus Transport. </p>		<p> Captain Fischer's Detachment. </p>		<p> Major Transport. </p>		<p> TOTALS. </p>		<p> REMARKS. </p>	
<p> 1803. March 11 </p>		1	1	<p> Three of these cases proved not contagious. </p>			
<p> 12 </p>		1	1				
<p> 13 </p>		8	8				
<p> 14 </p>		5	5				
<p> 15 </p>		3	6	2	...	11				
<p> 16 </p>		2	2	5				
<p> 17 </p>		7	1	2	10				
<p> 18 </p>		1	2	3				
<p> 19 </p>					
<p> TOTALS </p>		18	2	27	2	44							

<p> DATES OF ATTACK. </p>		<p> Lieut. Colonel De Watteville's Detachment. </p>		<p> Janus Transport. </p>		<p> Captain Fischer's Detachment. </p>		<p> Major Transport. </p>		<p> TOTALS. </p>		<p> REMARKS. </p>	
<p> 1803. March 20 </p>		13	2	2	...	27	2	3	44	<p> Two of these cases proved not con- tagious. </p>			
<p> 21 </p>		2	1	5				
<p> 22 </p>		2	3				
<p> 23 </p>		1	1	2				
<p> 24 </p>					
<p> 25 </p>		1	1				
<p> 26 </p>		1	1				
<p> 27 </p>		1	1				
<p> March 28 } to April 4 } 5 </p>		<p> 1st Quarantine begun. </p>			
<p> TOTALS </p>		24	2	28	6	60	<p> 2nd Quarantine begun. </p>						

No. 2.

GENERAL RETURN of the loss sustained by the British Army from Plague, during the conquest and occupation of Egypt:—viz. from the 8th March, 1801, to the 8th March, 1803.

PERIODS.		Attacked with Plague.	Discharged cured.	Died.	REMARKS.
From.	To.				
8th March, 1801	30th June, 1802	612	278	334	{ Including all the Casualties from Plague in the Indian Army. { Of these, 15 belonged to the British, and 33 to the German Regiments.
1st July, 1802	8th March, 1803	48	21	27	
TOTALS.		660	299	361	

No. 3.

Return of Persons belonging to the British Army and Transport Service, detained in Egypt in consequence of the Plague having appeared among them; with the Casualties that occurred from the 8th March, (the day the first case manifested itself) to the 28th April, 1803, when the Troops were reimbarbed for Malta.

DETACHMENTS AND SHIPS.	Number on board the 8th March, 1803.	Joined at Camp.	Total.	Attacked with Plague.	Died.	Remained on the 28th April, 1803.			
						Convalescents sent to Alexandria to Hospital.	Attendants.	Embarked for Malta.	Total.
Lieutenant Colonel De Watteville's Detachment	155	...	155	22	16	6	1	132	139
Janus Transport	19	...	19	2	2	17	17
Captain Fischer's Detachment.....	55	...	55	27	20	7	1	27	35
Major Transport	17	...	17	6	2	4	...	11	15
Persons from the former Quarantine.....	...	8	8	...	1	2	1	4	7
Medical Staff and their Servants.....	...	5	5	2	3	5
TOTALS.....	246	13	259	57	41	19	5	194	218

N.B. One of the Convalescents (Serjeant Sharpis) died of Hamorrhage in the Hospital at Alexandria; the others recovered.

IX. *Some Observations on the Nature and Treatment of the Calculous Diathesis.* By WILSON PHILIP, M.D. F.R.S. ED. &c. Communicated by DR. BAILLIE.

Read at the COLLEGE, November 5, 1819.

Worcester, June 20, 1819.

IN the year 1792 I published an account of some experiments made with a view to ascertain the states of the system which dispose to calculous disorders. Two of the most eminent writers on the subject, Dr. Marcet* and Dr. Henry†, having referred to these experiments, I am induced to lay before the Royal College of Physicians a short statement of the results afforded by them, when compared with the observations which, since the above

* An Essay on the chemical history and medical treatment of Calculous Disorders, by Alexander Marcet, M.D. F.R.S. &c. Page 186, et seq.

† Dr. Henry's Dissert. Inaugur. De Acido Urico 1807. Page 38, et seq.

period,

period, I have had opportunities of making on the diseases to which they relate. I am also influenced in presenting these observations to the College by the publication of a work on gravel, in many respects very excellent, by M. Magendie, lately translated into our language, which, from the celebrity of its author, is likely, both here and in other countries, to influence the opinion of medical men.

It appears to me, that M. Magendie has not sufficiently weighed all that has been done in this Country respecting this disease ; that from too confined a view of its phenomena, he has deduced a chemical hypothesis respecting its immediate cause, which is not easily reconciled with some well ascertained facts. He seems also to make too little account of the influence of the vital powers of the animal body, which so wonderfully modify the chemical and mechanical agents employed in it ; and what is of

of most consequence, may, as far as I can judge, lead to erroneous practice. Because azote is a component part of lithic acid *, and those, who use a large proportion of animal food and fermented liquors, are liable to calculous disorders ; and because he found, that when he confined animals to food which contains no azote, and thus

* The following is the result of M. Berard's analysis of lithic acid adopted by M. Magendie.

Azote . . .	39.16
Carbon . . .	33.61
Oxygen . . .	18.89
Hydrogen . .	8.34
	<hr/>
	100.00
	<hr/>

The result of an elaborate analysis of this acid by Dr. Prout, as given by Dr. Marcet in the 73d page of his work just referred to, is

Hydrogen . .	2.22
Carbon . . .	40.00
Oxygen . . .	26.66
Azote . . .	31.12
	<hr/>
	100.00
	<hr/>

thus deranged all the secretions, by destroying the health, no lithic acid was formed, he infers, that the secretion of this acid depends on the azote received in alimentary substances. But in so morbid a state, how many causes may exist to derange the secreting power of the kidney, and how many causes of derangement there may prevent the formation of lithic acid, even on the supposition that azote is supplied from some other source, by the skin for example from the atmosphere, are circumstances which do not appear to me sufficiently to have engaged the attention of M. Magendie ; and in speaking of the causes of gravel, he seems to forget that there are many things both in a full and low diet, which may be supposed to influence the state of the urine, besides the quantity of azote in the ingesta. It would too much anticipate the observations which I am about to lay before the reader, to state here other objections

objections to his opinion, which these observations will suggest.

My object in the above mentioned experiments was confined to one part of the subject; to ascertain the circumstances which dispose to the first formation of calculous concretions in the kidneys with a view to prevent a disease, whose consequences so often baffle all our efforts. These concretions I regarded as, almost always in the commencement of the disease, consisting of the lithic acid; and therefore conceived, that if we could ascertain the circumstances which tend to produce a deposition of this acid in the urinary passages, my object would be attained.

This opinion I believe is correct. There are few instances in which the concretions, formed in the kidneys, and passed in cases of gravel, consist of any thing else but lithic acid united to more or less animal matter. M. Magendie says, that he has examined
them

them above thirty times, and never found in them any other ingredients; and Mr. Brande observes, in a paper published in the Philosophical Transactions for 1808 “It appears from the preceding observations that calculi, formed in the kidneys and immediately voided, are almost always composed of uric acid.” It is not however to be overlooked, that calculi formed in the kidneys now and then consist of other substances. Dr. Marcet mentions instances in which they consisted, some of oxalate of lime, some of the phosphats, and some of cystic oxyd. When such is their composition, the observations which I am about to make will not apply to the disease. This is so rarely the case however in what is called gravel, that in twenty-five years, I have not met with one instance, in which I had reason to suspect it. When stones are retained in the pelvis of the kidney till they are too large to pass into

the bladder, different causes operate to occasion greater variety in their composition.

Although my inquiry related only to the circumstances which favour the precipitation of lithic acid, I could not avoid observing that another sediment often presented itself of a white or cream colour, sometimes tinged with red. Dr. Wollaston, Mr. Brande, and others have, since the above period, proved this sediment to consist chiefly of one or more of the phosphats, which by the accurate investigations of Dr. Wollaston have been ascertained to constitute a large proportion and sometimes the whole of calculous concretions, especially those of the bladder*.

I shall

* In the fifty-first page of Dr. Marcet's Treatise he observes in a note, " In a paper lately communicated to the Medico-Chirurgical Society by Dr. Henry, the author lays great stress upon the formation of calculi being essentially and originally a disease of the kidney, and he considers a lithic nucleus

I shall not detail my experiments at length, which would too much extend the limits of this paper, but only mention concisely such particulars as tend to establish the principles of treatment which seem best adapted to prevent the formation of calculous concretions in the kidneys. This is necessary as the original account of the experiments has long been out of print. I shall avoid mentioning the height of the thermometer, although this was generally stated in the original account, because I have no reason to believe that the small changes of temperature, which occurred during the experiments, sensibly influenced their results.

EXPERIMENT I.

A young man of about twenty years of age, in good health, and living

cleus from the kidney as by far the most common origin of all species of calculi.

in his usual way, partly on vegetable and partly on animal food, set apart for two days together, morning, mid-day, and evening, a certain portion of urine. All these portions at the end of forty-eight hours, had deposited some lithic acid. The whole deposited from those of each day was found to weigh one grain and a half. On the two following days he lived in the same way, except that he ate a lemon morning, midday, and evening. He set apart equal portions of urine. After each portion had stood forty-eight hours, the whole lithic acid deposited from the urine of each day amounted to three grains and a half. In this and the following experiments, the bladder was always emptied in the morning. The exercise on the different days of each experiment, and the portions of urine set apart, were always equal, except where the contrary is mentioned; and the vessels, containing the urine, were always of the

same kind and shape, and similarly placed.

EXPERIMENT II.

A boy about fifteen years of age, in good health, took for breakfast and dinner equal quantities by weight, of animal food, potatoes, and small beer. About midday and in the evening he set apart certain portions of urine. At the end of forty-eight hours they were found to have deposited no lithic acid, but in both there was some deposition of the phosphats. In the experiments which were first made, no attention was paid to the latter. It was not till a white sediment had frequently occurred, that my attention was attracted to it, and I endeavoured to ascertain the circumstances which tended to occasion or prevent its appearance. This will account for nothing being said of it in several experiments. Its precipitation usually takes place in a shorter time after the urine
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is discharged, than that of the lithic acid. On the following day, the boy lived in his usual way, eating animal food once a day, that the effect of the diet of the former day might go off. Next day, he ate for breakfast, equal quantities of bread and milk ; and apple dumpling for dinner. At the same times of the day he set apart similar portions of urine. At the end of forty-eight hours, I found in them some deposition of the phosphats and nearly one grain of lithic acid.

EXPERIMENT III.

This was made on a young man about twenty-one years of age and in good health. On the first day of the experiment he lived wholly on vegetable substances and milk ; and morning, midday, and evening he ate a lemon. At six and at ten o'clock in the evening he set apart portions of urine, which at the end of twenty-four hours had deposited two grains of lithic acid.

Next

Next day he ate no lemons and dined chiefly on animal food. At the same hours he set apart the same quantities of urine. At the end of twenty-four hours, neither had deposited any lithic acid.

EXPERIMENT IV.

Was made on the same person. On the first day he lived wholly on animal food and bread with water. No urine was set apart on this day. On the next day he lived in the same manner, and morning, midday, and evening set apart certain portions of urine. At the end of forty-eight hours, there was found in all of them a deposition of the phosphats, but no lithic acid.

Having lived in his usual way for a couple of days, that the effect of the above diet might go off, on the evening of the last of these days he ate a lemon. On the following day he lived entirely on vegetable substances,
except

except that he took a little broth at dinner. In the course of this day he ate two lemons. No urine was set apart on this day. On the following day he lived in the same way, eating a lemon morning, midday, and evening. At the same times of the day as in the second day of this experiment, he set apart similar portions of urine. At the end of forty-eight hours there was in all of them a deposition of lithic acid. These depositions put together amounted to about a grain and a half. There was no deposition of the phosphats in any of them.

EXPERIMENT V.

Was made on the same person as Experiment I. On the first day he lived wholly on animal food and water. On the second day, on bread, milk, and water, and ate a couple of lemons, one at eleven o'clock, the other at twelve. On the first day, portions of urine were set apart at midday, and
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at five o'clock p. m., and on the second day, similar portions at one and five p. m. Two days afterwards, all these portions of urine were examined at the same time. Those of the first day had deposited only five crystals of lithic acid ; those of the second day, a hundred and twenty crystals. In none was there any deposition of the phosphats.

The foregoing experiments seem to prove that considerable changes in diet influence the state of the urine, but more trifling changes have this effect. I have repeatedly observed that a single meal or two, more or less aced than usual, very sensibly affect the state of the urine. It is sufficient to relate the following instances, the result of which I have seen confirmed by many others.

EXPERIMENT VI.

A young man about nineteen years of age in good health, living partly on
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vegetable and partly on animal food, at two o'clock in the afternoon set apart a portion of urine. Next day he lived in the same way, except that he ate after breakfast a pound and a half of apples. At two o'clock he set apart a similar portion of urine. At the end of seventy-two hours I found in the latter two grains of lithic acid, in the former only one grain.

EXPERIMENT VII.

The young man mentioned in Experiment I. having supped and breakfasted on bread and milk, and after breakfast taken some acescent fruit, at two o'clock in the afternoon set apart a portion of urine, which, at the end of twenty-four hours, had deposited one grain of lithic acid. Next night he supped on beef and bread, and breakfasted on the same; on the following morning a similar portion of urine set apart at the same time of the day, at the end of twenty-four

four hours had deposited no lithic acid.

It appears from the foregoing experiments, that acid and acescent ingesta promote the deposition of lithic acid from the urine, while an opposite diet prevents it, and tends to produce that of the phosphats. I was led from these experiments to believe, that similar trials would always afford the same results; the following experiments however convinced me, that this is not the case.

EXPERIMENT VIII.

The subject of Experiment II. used for two days a very acescent diet. Three times during each day he set apart similar portions of urine. Those of the first day, at the end of forty-eight hours, had deposited three quarters of a grain of lithic acid, and also some of the phosphats. Those of the second day, at the end of the same length of time, had deposited a grain

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and a half of lithic acid, with a much smaller proportion of the phosphats. Having lived in his usual way for one day, on the following he lived wholly on animal food and water, and about the same times of the day set apart rather larger portions of urine. At the end of forty-eight hours I found in them a copious deposition of the phosphats, but no lithic acid. Thus far the result of this experiment tends to confirm that of the preceding ones. This day the whole of the fluid taken amounted only to four ounces, the urine being twenty-three ounces ; so great a check had perspiration suffered from the sickly disgust, of which the subject of this experiment complained, from living so much on animal food.

On the following day he lived on animal food, potatoes, and small beer, at the same times as on the preceding day setting apart similar portions of urine. On the morning of this day
his

his breath had a very sour smell, which even infected the air of his bed-room; and I could not prevail on him to continue the use of animal food alone. This smell of the breath went off towards evening. His urine exceeded the fluid drank, though not to so great a degree as on the former day. At the end of forty-eight hours, in the morning urine there was a deposition of lithic acid but none of the phosphats. In the second portion there was some deposition of both. In the last there was no deposition of lithic acid but a considerable quantity of the phosphats. The lithic acid deposited from the two first portions amounted to a grain and a half. It is remarkable in this part of the experiment, how quickly the urine was affected by acidity of the *primæ viæ*. The urine which was made in the morning, and consequently secreted while the breath was sour, deposited lithic acid, but none of the phosphats. The urine passed
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at midday, when the sourness had nearly gone off, deposited both lithic acid and the phosphats; while the evening urine made after the acidity had wholly disappeared for several hours, contained a deposition of the phosphats but no lithic acid. I never found a copious deposition of lithic acid and the phosphats in the same urine. Where there was much of either, little or none of the other appeared. The reader will observe that in the first and fourth days of this experiment, the quantity of lithic acid deposited was the same, although in the former, the diet had been wholly of a very acescent nature, consisting of bread, milk, honey, sour cream, and sugar; while in the latter, it was composed of a large proportion of animal food, and in other respects much more acescent. The same difficulty presents itself to a much greater degree in Experiment X.

EXPERIMENT IX.

Was made on the same boy. On the first day he lived chiefly on animal food, and towards evening began to complain of sickness. At the end of forty-eight hours, each of the three portions of urine, set apart on this day, contained a deposition of the phosphats but no lithic acid. On the following morning his breath had a sour smell which infected the air of his bed-room. He lived however nearly in the same way as on the former day. At the end of forty-eight hours, I found in the morning urine, and in that made early in the afternoon, a deposition of lithic acid, but none of the phosphats. The lithic acid in both portions amounted to a grain and three quarters. In the evening urine there was a deposition of the phosphats but no lithic acid. The result of this part of the experiment confirms, in a remarkable manner, that of the latter part of the preceding

ceding one. I constantly found in this boy, that confining him chiefly or wholly to animal food for more than one day produced great acidity of the *primæ viæ*, this acidity producing the same effects on the urine as acid ingesta do.

EXPERIMENT X.

Was made on the same boy. On the first day he lived wholly on beef and water. The fluid he took this day amounted only to eight ounces, his urine being twenty-four ounces. At the end of forty-eight hours, each of the three portions of this day's urine contained a deposition of the phosphats but no lithic acid. The next day he complained of nausea and great distaste for animal food, his breath having a sour smell. I prevailed on him however to live on this day as on the preceding. The fluid taken amounted to eight ounces, the urine to eighteen ounces. At the end
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of forty-eight hours in the first and second portions of the urine set apart on this day there was a deposition of lithic acid, in the third only a few crystals. In none of them was there any deposition of the phosphats. The lithic acid deposited from this day's urine amounted to one grain. The results of this day compared with those of the former show how much the deposition of lithic acid depends on the presence, and of the phosphats, on the absence, of acidity.

On the next day, the subject of this experiment lived in his usual way, that the effects of the diet of the two preceding days might go off. On the following day he lived on very acedent food, and three times in the course of the day took an ounce of lemon juice mixed with sugar. His urine this day amounted to nine ounces, the fluid taken, including lemon juice, to thirty ounces, so much had the lemon juice and vegetable diet, which he was

fond of and digested without any dyspeptic symptom, increased the excretion by the skin, for he had no alvine discharge this day. At the end of forty-eight hours, I found in each portion of this day's urine, very little lithic acid, but a copious deposition of the phosphats.

On the following day he lived in the same way. His urine this day, the bowels being still rather constipated, amounted to twelve, the fluid taken to sixteen, ounces. At the end of forty-eight hours, in each portion of this day's urine, I found a copious deposition of the phosphats, but no lithic acid.

It appears from this experiment that the diet may be of the most acescent kind, and even a large quantity of acid taken without occasioning any deposition of lithic acid ; and it is remarkable that in several parts of the foregoing experiments, the deposition of lithic acid was greatest when the urine

was most copious, in opposition to the observation of M. Magendie, that increased perspiration, as well as all other causes lessening the urine, tend to increase the deposition of this acid. That it is the less liable to be deposited, the greater the quantity of water in which it is dissolved, must of course, *cæteris paribus*, be true. We are to inquire into the cause which, notwithstanding the urine being so copious, here produced a precipitation of lithic acid, and wholly prevented it when the water secreted by the kidneys was so much lessened, and that even where acescent ingesta alone were taken. On considering the circumstances of Experiments VIII. and X., I could only ascribe this to the increase of insensible perspiration when the diet was acescent. This suggestion induced me to make the following experiments.

EXPERIMENT XI.

A young man, who was confined by
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rheumatism, repeatedly set apart half a pint of his urine, which I found, at the end of twenty-four hours, had always deposited about two grains and a quarter of lithic acid. I gave him in the morning about a scruple of the compound powder of ipecacuanha, which soon induced a copious perspiration. In the evening he set apart about a pint of urine, all that had been secreted for twelve hours, while the sweat still flowed. At the end of twenty-four hours, it had deposited no lithic acid, but a considerable quantity of the phosphats.

EXPERIMENT XII.

Was made on a man about fifty years of age, also labouring under rheumatism. In the morning he took twenty-six grains of compound powder of ipecacuanha. In about half an hour the sweat broke out and continued to flow freely all day. As he did not complain of thirst, I allowed him

him to take only his usual quantity of liquid, which did not much exceed a pint. At six o'clock in the evening, he set apart seven ounces of urine, while the sweat still flowed copiously. At the end of forty-eight hours, I found in it, no lithic acid, but a copious deposition of the phosphats. His urine this day was less than half its usual quantity.

Next day when he was going about, for his complaint did not confine him, at the same hour he set apart a similar portion of urine, in which I found, at the end of forty-eight hours, a copious sediment of lithic acid, weighing about three grains, but no deposition of the phosphats. He had taken about the same quantity of liquid on this day as on the former, but the urine was much more copious.

EXPERIMENT XIII.

The subject of Experiment I. took
eight

eight grains of the compound powder of ipecacuanha, which kept out a sweat during the whole of the day. After it had continued for about six hours, a portion of urine was set apart, and about two hours afterwards, another. In both, at the end of twenty-four hours, I found depositions of lithic acid amounting when put together only to a quarter of a grain. He had during this day drank two pints of milk and water. His urine amounted to only one pint. On the following day he lived in all respects in the same way, except that he underwent no diaphoresis. The urine did not exceed a pint, owing probably to the great abstraction of moisture on the preceding day. At the same times of the day similar portions of urine were set apart. At the end of twenty-four hours, they had deposited three grains of lithic acid. The first day's urine was of the deepest colour.

EXPERIMENT

EXPERIMENT XIV.

Was made on the subject of Experiment XII. The result of this experiment was, in all essential respects, similar to that of the last.

Although the foregoing are the only cases of the kind, of which I have notes, I have very often seen the same effect from sudorifics ; having never examined the urine during their operation after it had been kept for the proper time, without finding in it a deposition of the phosphats, and little or none of lithic acid.

The following experiments were made with a medicine increasing insensible perspiration only.

EXPERIMENT XV.

Was made on the subject of Experiment VI. He took, in small doses between twelve at noon and six in the evening, a grain and a half of tartrite of antimony ; and at ten set
apart

apart a portion of urine, which, at the end of twenty-four hours, had deposited no lithic acid. On the following day he lived in the same way as on this, except that he took no medicine. A similar portion of urine, set apart at the same time of the evening, at the end of twelve hours, had deposited lithic acid, which was neglected to be weighed.

EXPERIMENT XVI.

Was made on the subject of Experiment II. While living in his usual way, he ate four apples after breakfast, and at two o'clock set apart a portion of urine, which, at the end of twenty-four hours, had deposited a grain of lithic acid. On the following day he lived in all respects as on this, except that during the morning he took in small doses, one grain of *tartrate* of antimony, which produced no sensible effect. A similar portion of urine, set apart at the same time of the

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the day, at the end of twenty-four hours, had deposited no lithic acid.

EXPERIMENT XVII. & XVIII.

The foregoing experiment was twice repeated on the subject of Experiment I. In both instances the urine deposited two grains of lithic acid when no tartrate of antimony was taken. When this medicine was taken it deposited in the first instance, a quantity too small to be measured by the nicest balance, in the other instance none. According to the best of my recollection, when the tartrate of antimony was taken, there was generally a deposition of the phosphats, but, for the reason already given, no notice was taken of it in my notes.

The following difference in the effects of tartrate of antimony and compound powder of ipecacuanha deserves to be noticed. The effect of the latter on the urine is transitory, apparently ceasing as soon as the sweat ceases to flow.

flow. That of the former may generally in a greater or less degree, be perceived for several days after it is taken, during which it still seems to lessen the tendency of the urine to deposit lithic acid. I have also repeatedly observed, that the deposition of lithic acid was not so effectually prevented by tartrite of antimony, when it produced nausea, as when no sensible effect was experienced from it, a fact which we shall find explicable on principles we shall presently have occasion to consider.

We are acquainted with few medicines which more powerfully promote the secretions than mercury. I have had several opportunities of observing its effects on the urine.

EXPERIMENT XIX.

The subject of this experiment, a young man, had occasion daily to rub into the skin, a dram of strong mercurial ointment, after I had ascertained
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by repeated trials that a pint of his urine generally deposited, at the end of twenty-four hours, four grains of lithic acid and seldom any of the phosphats. After he had used the mercurial ointment for a few days, remaining at home and making no change in his diet, his urine was reduced to half its usual quantity, and deposited no lithic acid, but a copious sediment of the phosphats. This state of the urine continued for about a fortnight. He was obliged from his state of health to continue the use of the ointment, which soon after this affected his mouth, and his stomach and general health began to suffer from it. His urine now became more copious and again deposited lithic acid, with little or none of the phosphats. It seems from the foregoing circumstances, that at first the medicine, acting as a diaphoretic, as appears from the diminished urine, prevented the deposition of lithic acid, and occasioned that of the phosphats ;

phosphats ; but as the debility of the system, and particularly that of the stomach, which so intimately sympathises with the skin, came on ; the latter organ became less active, a greater flow of urine and the changes in its depositions just mentioned taking place.

The preceding appearances I witnessed a second time. The patient was advised to discontinue the use of the ointment and try the effects of a fuller diet. His general health improved, and on returning to the use of the ointment, the scanty urine with a want of the lithic sediment, and an increase of that of the phosphats, again occurred ; but these effects were neither so great nor permanent as in the first instance, the functions of the stomach and the general health being sooner impaired.

Some time after this the ointment produced a powerful diuretic effect. The urine was then pale, and deposited
less

less lithic acid, evidently in consequence of the greater proportion of water passing by the kidneys. It is proper to observe that while the urine was scanty, the bowels were rather constipated. The perspiration in this patient was more promoted by the external, than the internal use of mercury, although the latter produced no cathartic effect; which probably arose from the greater degree of dyspepsia which never failed to attend this use of it.

EXPERIMENT XX.

Was made on the subject of Experiment II. who had occasion to take small doses of calomel. For two days previously, he lived wholly on acescent food, three times a day setting apart portions of urine. Those of the first day, at the end of forty-eight hours, contained some deposition of the phosphats, and three quarters of a grain of lithic acid. Those of the second day,
little

little of the phosphats, and a grain and half of lithic acid. For the two following days he lived in his usual way, partly on animal, and partly on vegetable food. On the evening of the last of these days, he took a grain of calomel, which was repeated morning and evening for four days. On the two first, he lived as usual, setting apart no urine; on the two last, wholly on acescent food, and three times on each of these days, set apart portions of urine. In those of the first, I found at the end of forty-eight hours, a deposition of the phosphats, but none of lithic acid. In the first of those of the last day, at the end of the same length of time, there was a little of the phosphats, and half a grain of lithic acid; in the other, two more of the phosphats and no lithic acid. The result of this experiment would have been still more conclusive in a person the action of whose skin was not so easily increased, for in this boy the
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acescent diet alone had that effect in a remarkable degree.

The result of the foregoing experiments is more decisive on account of the diminished quantity of urine under the operation of diaphoretics. Thinking it merely possible that when the flow of urine is increased, the quantity of lithic acid secreted may be increased in the same proportion ; and wishing to take nothing for granted, I made several experiments, from which I had reason to believe, that the quantity of lithic acid is little influenced by that of the water secreted by the kidneys ; and consequently that, *cæteris paribus*, the tendency to deposit lithic acid, is less in proportion as the quantity of urine is greater.

As in the preceding experiments, we see the effect on the urine of causes increasing the action of the skin, in the following we see that of causes lessening the action of this organ.

None

None of these are more powerful than indolence.

EXPERIMENT XXI.

The subject of Experiment I. was confined by a rheumatic affection of the head, without fever. His diet for several weeks consisted wholly of beef-tea and calfs-foot jelly ; any thing more solid aggravating the pain of the head, yet during this confinement half a pint of his urine, at the end of twenty-four hours, deposited about two grains of lithic acid, as I ascertained by repeated trials, which I found to be more than twice the quantity deposited from the same quantity of urine, when he used moderate exercise ; nor could I, while he continued to take exercise, by the most acescent diet increase the deposition of lithic acid to the quantity above mentioned. That the result of the foregoing experiment might not be ascribed to the effect of indolence

indolence on the kidney, causing it to separate a less proportion of water, he was made to drink a considerable quantity of water, but the urine still continued to deposit more lithic acid than usual.

EXPERIMENT XXII.

The subject of the last experiment while in good health submitted to the following. He remained at home for two days without exercise, and found that half a pint of urine, passed on the second day, had deposited, at the end of twenty-four hours, two grains of lithic acid, that is, as I have already had occasion to observe, more than twice the quantity it deposited while he took his usual exercise.

Although the foregoing are the only cases of this kind of which I have notes, I have very frequently had occasion to observe the effect of indolence in increasing the deposition of lithic acid from the urine. I have also re-

peatedly observed, that it not only deposits more lithic acid, while exercise is avoided, but that if the state of indolence be of some continuance, a considerable time elapses after a return to exercise, before the deposition of lithic acid is reduced to its usual quantity. I found the urine deposit more lithic acid than usual for upwards of two weeks, after a long confinement.

The foregoing experiments seem to leave no room to doubt that the deposition of lithic acid from the urine is influenced by the state of the skin. We know that an acid passes by sensible perspiration. That it also passes by insensible perspiration, appears from the following experiment.

EXPERIMENT XXIII.

A piece of paper, stained with litmus, was tied about the neck. After it had remained there eight hours, during which time there was no sensible

sible perspiration on any part of the body, I found it changed to a red colour. This experiment was repeated, the paper being applied to the skin only for four hours, with the same result.

In an anonymous pamphlet, published in 1786, entitled, "*A Treatise upon Gravel and upon Gout, in which the sources of each are investigated, &c.*" afterwards claimed, and in 1793 republished, by Mr. Murray Forbes, it is stated, that the addition of any acid, even the carbonic, to urine, occasions a deposition of lithic acid. I repeated this experiment, using the sulphuric, nitrous, muriatic, citric, and carbonic acids, and always found the result as stated in the above pamphlet. I also found that the addition of an acid prevented the deposition of the sediment, since ascertained to be some of the phosphats of the urine. These facts appear to throw much light on

the results of the preceding experiments, from which it appears,

1. That acid and acescent ingesta tend to increase the deposition of lithic acid from the urine, and to prevent that of the phosphats. Exper. 1, 2, 3, 4, 5, 6, 7.

2. That a diet, composed of a large proportion of animal food, tends to lessen the deposition of lithic acid, and to increase that of the phosphats. *ibid.*

3. That every thing, which promotes the action of the skin, tends to prevent the deposition of lithic acid, and to occasion that of the phosphats. Exper. 8, 9, 10, 11, 12, 13, 14, 15, 16, 17, 18, 19, 20, 21, 22.

4. That dyspepsia tends to increase the deposition of lithic acid and to lessen that of the phosphats, both by producing acidity of the *primæ viæ*, and by rendering the skin inactive. Exper. 8, 9, 10, 19.

5. That indolence has the same tendency,

tendency, both by inducing dyspepsia, and by lessening the activity of the skin in proportion as it impairs the vigour of the circulation. Exper. 21, 22, see also the observations after Exper. 22.

6. That an acid passes by insensible as well as sensible perspiration. Exper. 23.

We have just seen that an acid mixed with healthy urine occasions a precipitation of lithic acid, and prevents that of the phosphats. When therefore we find, in the preceding experiments, that more acid than usual in the *primæ viæ* is attended with a greater than usual precipitation of lithic acid, while that of the phosphats is prevented, we infer, that under such circumstances, more acid than usual passes by the kidney; and this inference seems confirmed by the fact, that whatever corrects the superabundant acid in the *primæ viæ*, prevents the deposition of lithic acid, and disposes to

to that of the phosphats. But an acid, we have seen, passes both by sensible and insensible perspiration, and it appears from the above experiments, that however much acid may prevail in the *primæ viæ*, if the perspiration be free, the deposition of lithic acid is prevented, and a tendency to that of the phosphats produced. From these facts the inference appears to be unavoidable, that more or less of the acid, which, when perspiration is languid, passes by the kidneys, occasioning a deposition of lithic acid and preventing that of the phosphats, is, when the action of the skin is vigorous, thrown off by this organ.

I have often observed that it is more effectually thrown off by means increasing insensible, than sensible, perspiration ; which I would explain by the skin, in the former case, being more active ; in the latter, more relaxed. Similar observations seem to apply to the kidneys. The urine se-
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creted during sleep, although often of a deeper colour, does not in general afford so copious a deposition of lithic acid, as that secreted during the early part of the day, when the functions are most vigorous. I have reason to believe from many observations made during the course of the above experiments, that more acid is passed both by the kidneys and the skin in the day, than in the night ; during which some accumulation of it, although the diet may be very little of an acescent nature, takes place ; to be thrown off by the return of a more vigorous action of these organs. Even the debilitating effects of nausea seem, as appears from what is said above, to lessen the power of the skin to throw off this acid. We may thus see how causes, debilitating the digestive organs, may at length give such a disposition to the precipitation of lithic acid from the urine, that this shall take place before it leaves the pelvis of
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of the kidney. The circumstances of Experiment XIX. well illustrate the effects of dyspepsia in disposing to a precipitation of lithic acid.

On comparing the results of the foregoing experiments the reader will perceive, that we have no reason to believe that more lithic acid passes by the kidneys, when we use an acescent diet, than one of a contrary tendency, for this supposition is not necessary to explain the appearances ; and, under every diet, if an acid be added to the urine after it is discharged from the body, a copious precipitation of lithic acid is produced. It only appears from these experiments, that in the former case, more than usual of some other acid, capable of precipitating the lithic acid, passes by the kidneys. Nor have we any reason to believe when we see the precipitation of lithic acid prevented by diaphoretics, that this acid is thrown off by the skin. We still find it precipitated from the urine on the addition

tion of any other acid. But here the diaphoretic has excited the skin to throw off the acid, which would otherwise have passed by the kidneys, and occasioned its precipitation. If these observations are well founded, how far from the proper mode of investigating this subject has M. Magendie deviated, in directing his attention chiefly to the constituent parts of the lithic acid, and proposing to prevent its formation by preventing the reception into the system of one of those parts. It is not the existence of lithic acid in the urine (which we have reason to believe is necessary to the healthy state of this fluid) but its precipitation, that we are called upon to guard against.

How far the foregoing view of the subject corresponds with every thing we know of the causes which dispose to gravel, namely, the various causes of dyspepsia and of impaired action in the skin, is too evident to require
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any comment. And that these causes are all such as tend to produce a precipitation of lithic acid, and prevent that of the phosphats, and that the causes which prevent the precipitation of the latter are a freedom from acidity of the *primæ viæ*, and a vigorous state of the skin, which are known to be unfavourable to the production of gravel, are circumstances affording a strong argument for the opinion, that this disease generally arises from a deposition of lithic acid.

We are not, from the composition of calculi taken either from the kidney or bladder, to judge of the state of the urine, which in the first instance disposes to their formation ; because after a concretion is formed, new causes operate in determining the nature of the additions made to it. The following observation of Dr. Marcet is very valuable, that a degree of putrefaction taking place in the portion of urine which remains attached to the
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surface of calculi, and occasioning an evolution of ammonia, necessarily tends to prevent the deposition of lithic acid, and produce that of the phosphats. It is probable also, that the diseased state of the cavities, where calculi are lodged, often influences the depositions from the urine, and consequently the composition of the calculi. But in arranging a plan of prevention, the object is to trace the disease to its source ; and the more all the foregoing circumstances are considered, the more, I think, we shall be inclined to agree with Dr. Henry and Mr. Brande, that, in by far the greater number of cases, it originates in a deposition of lithic acid.

The success of the plan of treatment founded on that opinion is the most important, and one of the strongest arguments in its favour. I have long employed this plan, and I think in almost every instance, where there was reason to believe that no concretion had

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had been retained, the recurrence of gravel was prevented while the patient conformed to it. It consists in such an attention to diet, exercise, and a free action of the bowels, as tends to remove and prevent dyspepsia, and to support a due action of the skin, with the use of antacids, of which I have found soap the most effectual.

Sir Everard Home and Mr. Hatchet suggested the use of magnesia as a substitute for alkalis in gravel*. This Dr. Marcet observes “is an useful addition to the medical treatment of calculous disorders. Magnesia being less offensive to the stomach and yet capable of removing acidity from the digestive organs, an inconvenience ge-

* See a paper by Mr. W. T. Brande in the Phil. Transactions for 1810. This paper contains many good observations on the effects of magnesia and the alkalis on the urine. The superior effect of the magnesia in some of the cases, I should think, is in some degree to be ascribed to its cathartic property.

nerally accompanying the calculous diathesis ; it is often found advantageous to use it in long protracted cases, rather than caustic or subcarbonated alkali, the constant use of which would ultimately injure the stomach. Yet such is the tendency which the public has to overrate the utility of a new practice, or to take a mistaken view of its proper application, that there is every reason to believe that the use of magnesia has of late years become a frequent source of evil in calculous disorders." " This earth," he observes a little lower, " being the base of one of the most common species of calculi, the ammoniaco-magnesian phosphat, there is nearly an even chance, when magnesia is prescribed without any previous knowledge of the nature of the calculus, that it will prove injurious ; not only by affording the principal element of that calculus, but also by neutralizing in the *primæ viæ* any portions

tions of uncombined acid, by means of which the calculous matter might have been held in solution." This is an objection which evidently does not apply to the means of prevention which I am at present considering, and although the alkalis are certainly the most effectual means of correcting acidity, magnesia, both from its innocence when used moderately, and its forming with the acid of the *primæ viæ* a mild cathartic salt, I have occasionally found a very useful addition to other means. The risk of its forming concretions in the intestines may be obviated by mixing with it a small portion of rhubarb finely powdered, the taste of which may be concealed by the addition of some aromatic. By this mode of giving the magnesia, a common form in prescribing it for children, its cathartic effect is little if at all increased, and it is often rendered more grateful to the stomach.

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It is, I conceive, by removing acid from the *primæ viæ*, relieving the digestive organs from a load, and rousing them to better action, that cathartics are often of great use in obviating the tendency to gravel. “There is a circumstance,” Dr. Marcet observes, “in the history of calculous disorders which, though in a great degree unexplained, well deserves to be noticed. I mean the effect which a brisk purgative often has, not only in promoting the discharge of calculous matter or gravel, when it is small enough to pass through the natural passages, but also in giving a temporary check to its formation.”

We may also on the same principles, and particularly by the influence of the state of the skin on the urine, explain many other circumstances respecting the disease, which appear at present not to be generally understood. “It is for this reason,” M. Magendie observes, “that in gravel, a disease
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the treatment of which may in some degree be explained, we often find it necessary to employ means, whose efficacy we are taught by experience, although our theory of the disease does not enable us to explain their operation. Most patients for example derive advantage from means which correct the dyspepsia that attends this disease. Among these small doses of magnesia, rhubarb, the bark and sulphureous waters taken internally are frequently successful. I have seen purgatives given, so as to produce considerable evacuations, attended with the best effects. I have also observed good effects from cold and sulphurous baths, frictions and fumigations in the way they have been employed for some years past at Paris. Means of this description, all of which excite the action of the skin, rendering the perspiration more abundant, would seem from theory to be contrary to the indications in gravel, and in most cases do harm;

but in others, they are advantageous, and theory must submit to experience. The same observation applies to the sudden cure of gravel by a residence in the country, by a change of habits, occupations, affections, &c., no explanation of which can at present be given." The explanation of the effect of such circumstances is not difficult, when we consider the intimate connection of gravel with the state of the digestive organs ; and with respect to some of the foregoing means often, in the opinion of M. Magendie, doing harm in this disease, there is reason to believe he is here not wholly uninfluenced by his hypothesis respecting its nature.

M. Magendie indeed mentions other facts in direct opposition to this hypothesis. " We on the contrary," he observes, " see people, who from their diet and mode of life ought to be free from gravel, liable to it. These instances are rare, but not the less

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certain. Among the poorer inhabitants of a district between Tunbridge Wells and Lewes in Sussex, gravel is common, although they live almost wholly on vegetable food and hard beer." He admits that the inhabitants of warm climates are little subject to gravel, but explains this by alleging that they chiefly use vegetable food. In many of the most sultry climates however, we occasionally find the inhabitants using a large proportion of animal food, particularly fish, without observing that they are at all more liable to this disease. Many barbarous nations live wholly on animal food, but we are not informed that they are particularly subject to gravel. We have reason to believe that the disposition to this disease would be more general than it is in cold climates, were it not that cold applied to the surface with a due degree of exercise, acts not only as a tonic on the system in general, but as a powerful stimulus to

to the skin, a fact which any one may easily ascertain in cold weather, by comparing the quantity of urine with that of the fluid drank. Thus it is that the indolent of cold climates are most liable to this disease.

Nothing, as far as I can judge, can demonstrate more forcibly than the following passages the mistaken view of the nature of gravel taken by M. Magendie. "I do not," he observes, "consider the dyspepsia and other chronic diseases that frequently attend gravel, as connected with its cause, for every thing inclines us to think that when dyspepsia and gravel exist together, they are the effects of a common cause." In another place he remarks, "It is almost unnecessary to add, that all the causes which diminish the quantity of urine, such as copious sweats and other fluid evacuations are favourable to the formation of gravel by rendering the solution of the uric acid more difficult." And in

enumerating the causes of gravel he mentions copious perspiration as one. The following sentiments of Dr. Marcet on the subject of the first of these quotations are very different. “ It appears exceedingly probable on reviewing all the phenomena of calculous disorders, and in particular, on recollecting the benefit which is frequently experienced in this disease from the use of cathartics, and also from that of various tonics, that these affections most frequently originate from a deranged state of the digestive organs; and that such remedies as may fail in being of use as chemical agents, may still often prove beneficial by their tonic, or stimulating effects.”

Upon the whole, reviewing all that has been laid before the reader, I cannot help thinking that we have reason to believe, that gravel generally originates in a precipitation of lithic acid in the kidneys, in consequence of a greater than usual quantity of another
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ther acid, generated chiefly in the *primæ viæ*, passing by these organs; and that the best plan of prevention is to correct the tendency in the *primæ viæ* to form this acid; to support, by means which invigorate the powers of circulation, the action of the skin, by which in health any superabundance of acid is thrown off; and, when we find that notwithstanding such measures, too much acid still passes by the kidneys, to correct it by antacids before it enters the circulating fluids.

X. *A Contribution towards solving the disputed Question " what is the Nature of the Process called the Spontaneous Evolution of the Fœtus ?" By ROBERT GOOCH, M.D. Physician to the Westminster and to the London Lying-in Hospitals, and Lecturer on Midwifery at St. Bartholomew's Hospital. Communicated by the Registrar.*

Read November 5, 1819.

It was formerly believed when the arm came first, in labour at the full time, that the child could neither be expelled nor extracted in that position ; and that it was always necessary for the practitioner to introduce his hand, to bring down the feet, and thus convert an arm presentation into a foot presentation. Dr. Denman first observed, that in these cases, nature sometimes effects the delivery, by the expulsion of the breech. In this process he supposed that the arm recedes to the fundus of the uterus ; that as
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the breech comes down, the arm goes up, like the ends of a see-saw ; that an arm presentation is thus spontaneously converted into a breech presentation, and the process he named “ the spontaneous evolution.”

It has been generally thought that Dr. Denman over-rated the frequency, and consequently the probability of this occurrence : in Glasgow which contains ninety thousand inhabitants, Mr. Burns could not hear of more than one case : it was therefore considered too rare and improbable to have any influence on our practice in arm presentations ; but with reference to the process itself, no one suspected, until lately, that Dr. Denman had given an inaccurate account of it. The ascent of the arm into the uterus was considered an essential part of the spontaneous evolution and was described, in the cases published, not as a thing to which especial attention had been paid, but rather, it seems to me,

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as a thing of course. The person who spoke most distinctly and expressly to this point was Professor Boer of Vienna.

An arm presentation was brought into the practical school of midwifery, (a large lying-in hospital at Vienna) in the summer of 1801. The patient was placed on the delivery bed, and Boer prepared to turn, but having introduced the points of his fingers into the vagina, he was surprised to find the hand of the child higher than it had been when he examined before, (then, he had found it so low, that the fingers were easily seen, at the outer orifice.) "As the pains continued and grew stronger," says he, "I rested with my hand in the cavity of the pelvis. The action of the uterus continued, while, to my surprise, the fore-lying arm of the child distinctly moved up, on the right side of the vagina. What attracted my attention still more was this, at that moment, the whole cavity
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of the pelvis was filled with the whole breech of the child, so that my hand was compressed between the fore-lying arm and the breech of the child, or rather its left hip. The pain did not cease, and I could do nothing better than suffer my hand to advance with the protruding breech. What further happened to the fore-lying arm, whether it went completely up on the breast against the side of the head, whilst the breech passed through the lower aperture, or whether it came with the hand downwards, I cannot say, because of the progress of this rare delivery ; for the body and head of a fresh-living child went as quickly through the under aperture, as the breech some minutes before had come through the upper aperture into the cavity of the pelvis." (*Abhandlungen und versuche zur Begründung einer neuen Geburtshulfe, dritter Band. S. 57.*)

Though Professor Boer relates this case

case as an instance of spontaneous evolution, I think it most probable that it was no such thing. In every instance in which this process has taken place, the arm has been protruded up to the shoulder ; whereas in this, the fingers never descended lower than to become visible externally. I have been called in to turn, when the hand has been about as low, but on carrying it back into the uterus, I found the breech at the upper aperture, which soon afterwards began to descend in the pelvis. It was only a breech presentation, the hand having accidentally slipped down into the vagina, and I think it most probable that this of Professor Boer was a similar case.

A few years ago (1811) Dr. Douglas of Dublin published a pamphlet entitled "An Explanation of the Real Process of the Spontaneous Evolution of the Fœtus." He affirmed that Dr. Denman had given an inaccurate account of the process. According to
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Dr. Douglas, the arm and shoulder do not return into the uterus, but with the side of the thorax protrude through the os externum; thus, the side of the trunk comes to press on the perinæum; this makes room for the breech to descend, from the brim of the pelvis into the hollow of the sacrum, and by a few further efforts of the uterus, the rest of the body and the lower extremities are expelled, leaving the head and one arm still to extricate. This was, in the form of a general description, what Dr. Douglas had witnessed, in several cases: he adds “if the arm of the foetus should be almost entirely protruded, with the shoulder pressing on the perinæum; if a considerable portion of its thorax be in the hollow of the sacrum, with the axilla low in the pelvis; if with this disposition, the uterine efforts be still powerful, and if the thorax be forced sensibly lower during the presence of each successive pain, the evolution

evolution may with great confidence be expected."

Herder, an eminent practitioner, now living at Weimar, had several years before, observed the same thing. The work, from which I quote, is dated 1803. He describes himself as being called to a peasant's wife, who had been suffering, from Friday to Sunday, spasmodic pains, by which she had been greatly exhausted. "The left elbow with the left side of the chest presented; the navel string hung out, cold and pulseless; the right arm lay over the pubis; the head high on the right side; the feet above, in the fundus of the uterus, over the head; the breech lay on the left side;" he endeavoured to turn more than once, but was prevented by the immoderate spasms; he gave opium; waited; tried again, and failed. The opium which was given to quiet the pains, acted as a cordial, and she revived. "The patient now had strong pains, interrupted
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ed by few pauses. This state might have lasted a good half or three quarters of an hour, when she called out to me that the pains were so violent, every thing was coming away. On examining, I found to my astonishment, that the violence of the pains had thrust out, not only the left arm, but also the breast and a part of the belly of the child, through the pelvis and the external parts; the head and the pelvis as well as the feet of the child remained behind, and could not be expelled naturally. As the belly of the dead child was much distended with air, and was an insurmountable hindrance, in the pelvis, to the half-doubled and half-born child, I immediately perforated the distended breast and belly, took the viscera out, and then drew down the under part of the trunk, in order to get hold of the pelvis of the child; I reached it, and as soon as it was born, the feet, head, and right arm followed." The patient

tient did well. (Diagnostisch: praktische Beiträge, von W. G. Herder, page 50 to 57. Leipzig, 1803.)

Since the publication of Dr. Douglas's tract, Dr. Kelly of Newtown, Swords, near Dublin, has published a pamphlet on this subject (1816). He thinks that Dr. Douglas's account of the process is more inaccurate than that of Dr. Denman, and that the latter, though he has been wrong about the time, has been right about the fact, that the arm ascends into the uterus: it ascends, he says, not as Dr. Denman thought, *when* the inferior extremities are pressed down, and *while* the uterus is in contraction, but after they have been pressed down, and *when* the uterus next ceases to act. "The superior extremity goes up, the moment the uterus ceases to press it down, or in other words, at the termination of a pain."

Dr. Kelly's Essay is perspicuous
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and well written, yet I find in it no reason for supposing, that he has ever witnessed the process, the opposite opinions on which, he thus ventures to criticise. An estimable Reviewer in the Medical Repository thinks, that, "although the process described by Dr. Douglas is, particularly with small sized children, the more common method," yet that the evolution described by Dr. Denman occasionally occurs. In this unsettled state the question at present stands.

I was aware that such facts had been observed, and such opinions entertained, about this rare process "the spontaneous evolution," when on Sunday morning, the 24th of October, 1819 (the day on which I am writing) one of the midwives who attend the out-patients of the Westminster Lying-in Hospital, sent begging me to go to her as she had a case of great difficulty. The patient was a tall young woman, at the full time with her first child;

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she had had slight pains during the night ; about four in the morning the membranes had broken, and she had sent for the midwife. From her I learnt, that when she arrived, the orifice of the uterus was somewhat open, but that no part of the child could be felt. At length the pains became stronger; an arm descended, and she had sent off for further assistance.

The first thing I observed was, that not only the arm was out its whole length, but that the shoulder had turned forward under the arch of the pubis, like the occiput just before the head is born. The next thing I observed was, that when a pain came on, which was very strong, the side of the thorax pressed down with great force against the perinæum. Struck by these appearances, I abstained from turning, and set down by the bed side, fully expecting what actually took place, the spontaneous expulsion. Resolved to know what became

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came of the arm, if this should happen, and thus fit myself for a witness on this disputed point, I laid hold of it with a napkin and watched its movements: so far from going up into the uterus, when a pain came on, it advanced as well as the shoulder still forwarder under the arch of the pubis, the side of the thorax pressing more on the perinæum, and appearing still more externally; it advanced so rapidly that in two pains, with a good deal of muscular exertion on the part of the patient, but apparently with less suffering than attends the birth of the head in a common first labour, did the side of the chest, of the abdomen, and of the breech*, pass one after

* "The breech," says Dr. Douglas, "is not expelled exactly sideways as the upper part of the trunk had previously been, for during that pain by which the evolution is completed, there is a twist made about the centre of the curve at the lumbar vertebræ, when both buttocks instead of

after the other, in an enormous sweep over the perinæum, till the nates and legs were completely expelled. The head and other arm were still to be extricated, but this was effected with the greatest ease; the child was dead; the mother had not felt it move since the day before at noon; not only was the chord without pul-

the side of one of them are thrown against the perinæum, and immediately after the breech issues forth. The upper and back part of it appearing first, as if the back of the child had originally formed the convex, and its belly the concave side of the curve." Now though I have said that the side of the breech as well as of the belly and chest passed over the perinæum, I certainly cannot affirm it positively as I was too much occupied about the arm: this shows how liable we are without any intention to deceive, to describe things supposed as things observed. I can easily understand therefore, how those who considered the retrocession of the arm as an unquestionable part of the process, should describe the extraction of the head and one shoulder, as "the shoulders and head," and such things.

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sation, but it was empty and shrunk, and looked as if it had been some time since blood had circulated through it; the side of the chest which had come foremost was of a livid green, the skin peeled off, and the naked cutis was dark brown.

It is commonly supposed that the expulsion can be effected, arm first, only when the child is very small, or premature, or the pelvis very large; in the foregoing case there were none of these supposed requisites; the pelvis was not unusually spacious; the patient was at her full time; it was a first labour, and the child was large. Yet I do not wish to lay more stress on this case than it deserves. When I consider that the original theory of this process consisted rather in conjecture than observation, that the opposite opinion is founded on the latter kind of evidence, and when I find this opinion

exactly corresponding with my own experience, I confess it is with me nearly conclusive, but I do not expect it to produce the same effect on others: I offer it only as a contribution towards settling the question.

As presentations of the arm are not rare, yet so rarely terminate in spontaneous expulsion, it is an important question what occasions this result when it does occur. Experience has in some degree answered the question; the pains have been always unusually strong and efficacious; the child has been almost always dead, and it is easy to understand how a dead child should be easier bent and adapted for expulsion than a living one: but besides these, much I apprehend depends on the position of the child. In the spontaneous expulsion, the breech descends, by the back of the pelvis, and the hollow of the sacrum. For this purpose
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there can be little doubt that some positions of the child are more favourable than others; *the most favourable* would be that in which the shoulder is to the arch of the pubis, the head towards the front, and the nates towards the back of the uterus; *the least favourable* would be that in which the shoulder is to the sacrum, the head towards the back, and the nates towards the front of the pelvis. This last I should think must render it impracticable, but it is probable that any great deviation from the first position I have described would diminish in a great degree the chance of the spontaneous expulsion. How often the spontaneous expulsion is prevented by unfavourable positions of the child, depends on the solution of another question, that is what are unfavourable or impracticable positions, and how often do they occur? this is difficult to be solved by
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the experience of an individual, or by any trustworthy authority with which I am acquainted. I mention it only as a subject for future inquiry.

Those who do not practise midwifery will think that those who do, are spending many words on a trivial question, and that it signifies little whether in "the spontaneous evolution" the arm goes up or down; yet whenever nature surmounts an obstacle by means so preferable to those employed by art, the exact way in which she succeeds is an important object of inquiry. It was a knowledge of this circumstance, and of the signs by which it was indicated, which caused me to desist from turning, to expect the spontaneous evolution, and thus led to the easy and happy termination of the case which I have related. At this time it is impossible to say what influence a minute and accurate knowledge of this process, its causes, its mechanism,

mechanism, and the way to facilitate it, may have on our future practice in arm presentations.

In the present state of our knowledge, however, an acquaintance with this fact ought to have little influence on our practice. If the arm and shoulder protrude far, the pains are strong, and the thorax presses hard on the perinæum, it will be right to wait and watch for a little time; beyond this whoever suffers a knowledge of this rare fact to discourage him from turning in arm presentations, would be guilty I think of criminal irresolution.

XI. *On the Employment of Venæsection in Cases of sudden seizures, commonly called Fits.* By JOHN LATHAM, M.D. F.R.S. President of the Royal College of Physicians, &c.

Read at the COLLEGE, December 16, 1819.

THE indiscriminate use of the lancet in all cases of sudden fits from whatever cause arising, is a practice which physicians have long been in the habit of deploring. It is not however so much the fault of the medical profession as that of public opinion, which proceeding upon superficial observation that *some* fits have been relieved by venæsection, presses the practitioner to bleed in *all*. If following the dictates of sound sense and sober discretion, he should but for a moment doubt about the propriety of diminishing the quantity of the circulating fluids, the number (and it may be also the crowd of medical

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cal persons) drawn together by the sudden occurrence, forces him most frequently to act in opposition to his better judgment: if the patient recovers, the cure is attributed to the instant blood-letting, and if he dies the conclusion is too hastily made that nothing could have saved him. My opinion however is, that the cure, as it is called, is in most instances only a lucky escape from death, and that the conclusion so constantly acquiesced in (not to speak too harshly) is not the conclusion of science.

In cases of hysteria and of epilepsy, where the contortions and struggles of the patients will not always very readily allow the practitioner to employ his lancet, we usually find them afterwards not at all the worse for the omission; and yet these are cases where perhaps, if in any the operation is to be performed without previous consideration, venæsection might possibly have been of service.

vice. Even here, however, when the fits are over, the practitioner does not often regret the omission, for *afterwards* he seldom takes away blood, notwithstanding he would most undoubtedly have performed the operation during the paroxysm, if the arm of the patient could have been held steadily. If the omission then in consequence of such impracticability so seldom leads to any regret, I have a right to assume that if venæsection could have been actually practised, it might not have been followed by any more beneficial results.

In cases of syncope, from all the various causes which are known to induce it, the same indiscriminate practice is too often pursued. It would be superfluous to say what are the usual consequences in very debilitated systems, which are those most liable to this affection; for it cannot properly be denominated a disease, inasmuch as it is most commonly

monly a sign only that the heart is influenced by some sudden condition attendant upon debility, which for a while must necessarily produce a pause or suspension of its motion: a distinction which if always considered with attention might happily save the lives of thousands.

In apoplexy, although it must be confessed that letting blood is seldom so injurious as in those other cases to which I have been alluding, yet even here the practice is by much too indiscriminate and requires in my judgment more consideration than is usually bestowed upon it. It certainly ought to make some difference in our practice, whether the patient be of very full or of very slender habit, although the custom of bleeding is religiously observed equally in both, and a fancied *threadiness* or *sharpness* in the pulse is usually considered as a sufficient justification for the immediate loss of blood, notwithstanding

standing the sallowness of complexion and the flaccidity of muscle may indicate any thing rather than plethora. The lancet however has generally been employed, without any regard either to such circumstances or even to the age of the patient, long before the physician has arrived, and the temporary relief which has been obtained, (for some benefit is commonly experienced in the first instance) is used as an argument for the repetition of the operation, and it is well if by talking about *local* fulness and oppression of the brain he can carry the point so far as to satisfy the bystanders that by leeches to the temples, or by cupping glasses to the neck, a sufficient quantity of blood may be taken more immediately from the neighbourhood of the affected part, than by any depletion from the more general circulation.

But I must be more particular on this subject: I suppose a patient
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of corpulent habit, firm muscles, florid complexion, and a full feeder, suddenly seized with a fit, and insensible to every thing around him, breathing with considerable noise and difficulty. Under these circumstances there cannot be a question about large and immediate bleeding by the lancet from the arm, from the temporal artery, or from the jugular vein, from the temples by leeches, or from the neck or head by cupping; other means of depletion too by emetics, by purgatives and by clysters should as soon as possible be adopted, and blisters should be applied upon the head itself. It will *sometimes* happen that patients by such a vigorous method promptly put in force will soon be established in the full exercise of their usual functions, but oftener indeed that their faculties are only partially restored. But in systems directly the reverse, in constitutions flaccid and emaciated, it is surely contrary
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to sound discretion that a *similar* mode of treatment should be instituted. It may be indeed from oppression on the brain, that the sudden apoplexy has supervened, but we should recollect that congestions may also sometimes arise from the *slowness* of the stream, and from the *weakness* of the impelling force. We should take the trouble of inquiring into the circumstances of the patient's constitution before we proceed to an operation which, if misapplied, is fatal. We should reflect upon that which is constantly occurring in daily practice, and thence be taught that the cause which produces coldness of the extremities, and which arises not only from a deficiency of power in the circulation, but of the fluid also which is to be circulated, may in the delicate organization of the brain (long before such coldness is sensibly experienced in the extremities) be producing there also an affection which,
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it is obvious, that depletion, in the manner and to the extent commonly practised, cannot possibly be expected to remove.

It would be presumptuous in me to think of laying down rules applicable to every apoplectic case. It would indeed be impossible to do so, since circumstances must vary and determine the practice in almost every instance, but I would venture to state that in the absence of every other means of information, where a practitioner is called to a person in a fit (and the observation might be extended to other diseases also) he would find enough to determine him against venæsection, should the subject be emaciated and the muscles flaccid. No clamour or interference should then induce us to consent to the abstraction of that upon which life so mainly depends; and the relief which might be urged by any as being usually experienced in such cases, should

should be met with the observation, which in numberless instances I have been compelled to record, that in two or three days we shall want the fluid which we are now about so lavishly to expend. But it will properly enough be asked, do the persons *always* recover whom you refuse or omit to bleed? By no means: for to use an expression of our late lamented friend and colleague Dr. Pitcairn, upon the propriety of venæsection in the catarrhal affections of very delicate and of very old persons, “if you bleed them *you* kill them, and if you do not bleed them *they* die;” so in these cases of apoplexy which take place in very languid and delicate systems I answer, do not take away that from the circulation upon which the patient’s strength depends, and through which strength a cure, if a cure be possible, is only to be attained: the difficulties are great on either side, but do not diminish those

powers by which the several functions of the system are upholden; if the patient be already sinking in consequence of diminished force, assist the natural actions but do not weaken them: improve the only possible chance remaining for your patient, and when he dies, as will generally be the case, it will be some consolation for you to reflect that depletion has not destroyed him, and that by your own officiousness he has not been hurried into his grave.

From what I have said I hope not to be understood as meaning that, if I condemn indiscriminate bleeding, I am desirous that the physician should stand still over his patient and do nothing. It may be that congestions in the intestinal canal, causing pressure upon the larger blood-vessels which convey blood to and from the heart, may occasion an unequal distribution of it, and which may therefore be accumulated any where

throughout the system, and if in the head, there is consequently produced every symptom of apoplectic distress. It may be that flatulent distension of the stomach is producing the same effect, and here too I cannot help calling to every physician's recollection how the temporary suspension of the functions of the brain, during the common suffocating paroxysm of hysteria is relieved by any thing which will evacuate the flatus, and remove the distension. Shall we not from analogy then try similar means in those cases of extreme debility where abdominal congestion is so likely to happen, and which as I have above stated may bring along with it every apoplectic consequence? At any rate the prudent physician will here I think evacuate the intestines as well as the stomach before he proceeds under such circumstances to empty the blood-vessels.

But having evacuated the stomach
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and intestine canal, and removed any congestion that might there by its pressure upon the larger vessels become the cause of the apoplectic fit, shall the physician not then bleed? The circumstances which would not allow general depletion by venæsection, previous to the evacuations of the stomach and bowels, I presume will *usually* continue the same afterwards; but the physician will at least possess this advantage, that the time gained will have given him a fairer opportunity of weighing the probabilities on each side of the question: He will not have been driven hastily into the adoption of that which he might now have repented, and if he still thinks that his patient has not strength enough to bear the loss of blood, and that the vigour of the digestive organs is insufficient speedily to repair the expenditure, he will not even yet venture upon the lancet *generally*, but confine his efforts to

such *local* means of depletion as may be carried into effect by cupping-glasses, leeches and blisters.

What I have advanced respecting venæsection in apoplexy, is applicable also for the most part in paralytic attacks. The lancet has been employed here too without due discrimination, but as the patient is not *always* so entirely affected as to prevent him from interposing his own judgment, the weakness of his condition will often fortunately by himself be opposed to any very outrageous extent of its use. But indeed in these cases the generality of medical practitioners are not very urgent to employ the lancet, commonly contenting themselves with what may be termed a fair depletion by the intestinal canal after one previous bleeding, and then flying at once into the opposite extreme of stimulants and tonics: a practice which may be characterized as too general to be
never

never wrong, but yet too mechanical to be always proper. Sufficiently instructive however is the lesson which it affords us respecting venæsection, for although palsy be seldom attended like apoplexy with the total abolition of sense, yet it nevertheless usually depends upon an affection of the brain nearly allied to the same cause, from distension perhaps much more partial, or from effusion trifling and inconsiderable; but still palsy is so far from imperiously demanding even in the majority of cases any great loss of blood for its relief, that volatiles and aromatics and all the tribe of stimulants have immediately been resorted to (I will not say judiciously, although the patient may have escaped) without any previous depletion whatever.

But it is time to conclude these observations, which may I think be summed up in a few short lines: that venæsection is too generally employed,
and

and the relief obtained from it in the first instances usually fallacious ; that flaccidity of muscles which depends upon the inanition of the vessels which nourish them, is a better criterion of the actual state of the vascular system than the pulsation of the artery ; and that the unequal distribution of blood, which even in the weakest constitutions may affect the head, will in such debilitated habits be more safely and effectually relieved by other means of depletion than by general bleeding.

XII. *Observations on puerperal Insanity.* By ROBERT GOOCH, M.D. *Physician to the Westminster and to the City of London Lying-in Hospitals, and Lecturer on Midwifery at St. Bartholomew's Hospital. Communicated by the Registrar.*

Read at the COLLEGE, December 16, 1819.

It is well known that some women, who are perfectly sane at all other times, become deranged after delivery, and that this form of the disease is called puerperal insanity. My situation gives me more than the common opportunities of seeing it, and though I am unable to make any important additions to our knowledge of the subject, I have witnessed some things which seem to me to deserve relation: these I will venture to describe, together with what I have observed about the causes, progress, and treatment, of this distressing malady.

The

The most common time for it to begin is a few days, or a few weeks, after delivery; sometimes it happens after several months, during nursing, or soon after weaning; I have seen it at the commencement of pregnancy, and it is said sometimes to arise at the commencement of labour. (Denman).

The approach of the disease is announced by symptoms which excite little apprehension, because they so often occur without any such termination; the pulse is quick without any manifest cause, the nights are restless, and the temper is sharp; soon, however, there is an indescribable hurry, and peculiarity of manner, which a watchful and experienced observer, and those accustomed to the patient will notice, her conduct and language become wild and incoherent, and at length she becomes decidedly maniacal; it is fortunate if she does

not attempt her life before the nature of the malady is discovered.

When the disease appears under the form of melancholia, it commonly begins some months after delivery, and comes on more gradually; the patient has suffered in her health from nursing, experiences a failure of memory, confusion of mind, and an irresistible and inexplicable depression of spirits; she finds it difficult to think on any subject long, her domestic accounts bewilder her, she is dissatisfied about herself and full of anxiety. This state continues in a greater or less degree for several weeks; at length it becomes more marked, her countenance is mournful and downcast, she is silent and thoughtful, fancies that she has some serious disease, accuses herself of some moral depravity, and supposes herself an object of punishment and scorn.

It is needless, here, to give a minute and detailed description of mania
and

and melancholia in child-bed or suckling women; it is generally like mania and melancholia under other circumstances; but I may here remark (what those who have carefully observed the affections of the nervous system will readily acknowledge), that when once its functions are greatly disordered, there is no end to the diversity which the symptoms are capable of assuming; that this is sometimes the case in the disease of which I am speaking, was strikingly exemplified in the following instance.

Mrs. — is 29 years of age; she has long been unusually subject to the common forms of hysteria; I have seen her after being strongly excited in conversation sink down insensible, and a few minutes afterwards recover with choaking and sobbing; her husband tells me that he has often seen her whilst sitting at the dinner table become apparently insensible, with her eyes open, still sitting up, continue in
this

this state several minutes, and then come to herself again, but totally unconscious of what had taken place in the interval. She married nine years ago, has been pregnant many times, but has only borne one living child; every other time she has either miscarried during the early months, or what was more common, the child has died without any obvious cause about the 6th or 7th month, and premature labour has come on a week or two afterwards. A few days after her last delivery, of a dead child at the 7th month, (a circumstance which was attributed to some domestic agitation,) she was seized with a violent head and face-ache which was confined to the left side, and which subsided under the use of hemlock, but she continued to suffer flatulence of stomach, had a quick weak pulse, and was much depressed in spirits: one evening she told her husband that she had never discharged the duties of a wife

as

as she ought to do, and that her death would be a happy release both to him and her: the next morning she made an unsuccessful attempt to cut her throat. I now saw her in consultation with a physician of extensive experience in these diseases, she was put under the care of a regular attendant, and was at times so violent that it was necessary to confine her with a waistcoat.

A few days after our first visit we were summoned to observe a remarkable change in her symptoms; the attendants said she was dying, or in a trance; she was lying in bed motionless, and apparently senseless; it had been said that the pupils were dilated and motionless, and some apprehensions of effusion on the brain had been entertained, but on coming to examine them closely, it was found, that they readily contracted when the light fell upon them; her eyes were open, but no rising of the chest, no movement

movement of the nostril, no appearance of respiration could be seen; the only signs of life were her warmth and pulse; the latter was as we had hitherto observed it, weak, and about 120; her fæces and urine were voided in bed.

The trunk of the body was now lifted so as to form rather an obtuse angle with the limbs, (a most uncomfortable posture) and there left with nothing to support it; there she continued sitting while we were asking questions and conversing, so that many minutes must have passed.

One arm was now raised, then the other, and where they were left, there they remained; it was now a curious sight to see her, sitting up in bed, her eyes open, staring lifelessly, her arms outstretched, yet without any visible sign of animation; she was very thin and pallid, and looked like a corpse that had been propped up, and had stiffened in this attitude. We now
took

took her out of bed, placed her upright, and endeavoured to rouse her by calling loudly in her ears, but in vain; she stood up but as inanimate as a statue; the slightest push put her off her balance; no exertion was made to regain it; she would have fallen if I had not caught her.

She went into this state three several times, the first time it lasted fourteen hours, the second time twelve hours, and the third time nine hours, with waking intervals of two days after the first fit, and one day after the second. After this the disease resumed the ordinary form of melancholia, and three months from the time of her delivery she was well enough to resume her domestic duties*.

But

* Dr. Sutherland, with whom I attended this case, related to me several others, similar, but far more extraordinary, for the time which they lasted. One was a young lady who continued in this state for several months, and was preserved only by great vigilance and management in feeding her.

The

But to return to the ordinary occurrences of the disease. When once it has begun, its duration is very different, in different cases ; sometimes it

The other was a male hospital patient; being suspected of imposture he was one day placed upright at the edge of the cold bath, and at length gently pushed in; he fell to the bottom like a stone, and continued there without the slightest effort to save himself, till it seemed no longer safe to continue the experiment; after remaining in a cataleptic state for several months, he recovered.

In the descriptions of catalepsy to be found in medical writers of eminence, we are told that the "patient remains in the same posture which he was in when he was first seized," but that if a by-stander puts him into a new attitude, "the joints yield with a waxen flexibility and remain in the posture into which they are put." (Van Swieten.) Those, therefore, who have witnessed and believed in the reality of this strange affection, have concluded, that the power which keeps the voluntary muscles contracted remains, but that the power of changing and directing these contractions is taken away, Vol. v. § 1037.

There is something so marvellous in these appearances, and such singular instances of fraud are sometimes detected in our intercourse with the sick, that some have believed catalepsy to be
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it subsides in a few days, or even hours, but this is rare ; it commonly lasts several weeks, or even months ; Dr. Denman says, it never continues longer

a fabulous disease. When Mr. Abernethy delivered his Hunterian oration at the College of Surgeons, (a singular instance of the power of genius over a threadbare subject) he related the following anecdote of Mr. Hunter. “ A patient in the hospital feigned to be afflicted with catalepsy, in which disorder, it is said, a person loses all consciousness and volition, yet remains in the very attitude in which they were suddenly seized with this temporary suspension of the intellectual functions. Mr. Hunter began to comment before the surrounding students on the strangeness of the latter circumstance, and as the man stood with his hand a little extended and elevated, he said, you see, gentlemen, that the hand is supported, merely in consequence of the muscles persevering in that action to which volition had excited them prior to the cataleptic seizure. I wonder, continued he, what additional weight they would support, and so saying, he slipped the noose of a cord round the wrist, and hung to the other end a small weight, which produced no alteration in the position of the hand. Then, after a short time, with a pair of scissors he imperceptibly snipped the chord. The weight fell to the ground, and

longer than six; this is good authority for the general fact; yet a few years ago, in a large lunatic institution, I saw a young woman who had become insane

and the hand was as suddenly raised in the air by the increased effort which volition had excited for the support of the additional weight; thus was it manifested that the man was possessed with consciousness and volition, and the imposture stood revealed. (Abernethy's Hunterian Oration, p. 56.)

That in this case the patient was convicted of imposture I do not doubt; the only question is about the principle on which the conviction was effected. Mr. Hunter detects some remains of consciousness and volition, and thence infers the perfect possession of these faculties; but because we find that they are not completely extinguished, is it right to conclude that they must be completely unimpaired? Catalepsy is defined to be a loss of consciousness and volition, but medical definitions are only general descriptions, and seldom admit of rigid application. It may be said that voluntary movements are impossible without consciousness, and that where these movements occur there cannot be a loss of consciousness; but the loss of this faculty may be total, or may be partial; though volition is impossible in the former it is not so in the latter,

sane after lying-in; she had been there twelve months without recovering, and according to the regulations of the institution,

and who has ever said, for who could tell that in catalepsy, consciousness was utterly extinguished? Sleep is said to be a suspension of sensation, volition, and the intellectual faculties; yet if the sleeper moves, or manifests signs of outward sensibility, do we consequently deny the sincerity of his sleep? when we are tired of sleeping on one side, do we not turn to the other without waking? Sleep-walking is a still more remarkable instance of the kind; nay further, not only are voluntary movements compatible with a considerable loss of consciousness, but these movements are capable of being influenced by external impressions. If a person is tickled during sleep he will often put his hand to the part without waking; those who talk in their sleep often answer questions; the answer it is true may be absurd, but it relates to the question, and has been obviously influenced by it. Now as some slight remains of consciousness and voluntary motion does not lead us to conclude that a person is feigning sleep, so from these appearances alone we should not hastily conclude that a person is feigning catalepsy. Catalepsy like sleep may be more or less sound.

institution, was going out among the uncured. Out of ninety-two cases of puerperal insanity admitted into the hospital de la Salpetriere at Paris, two lasted two years, and thirty-six (that is more than one third) went uncured. (Abstract of a paper on this subject read by Monsieur Esquirol, to the Société de Medicine, in the Quarterly Journal of Foreign Medicine and Surgery, for November 1818, page 104.)

It is the common opinion that puerperal insanity is harmless to life; most cases it is true terminate favourably. "In general," says Dr. William Hunter, "this species of madness cures itself; I imagine that in all my business I have seen twenty or thirty of these cases, and all of them did well." "I have been called in, where a great number of stimulating medicines and blisters have been administered, but the patients have gone on talking nonsense till the disease has

gone off, and they have become sensible. It is a species of madness which they generally recover from, but I know nothing of any singular service in it." (M. S. Lectures) Of the cases which have occurred in my own practice, or about which I have been consulted, although they have excited great distress and apprehension, yet all, sooner or later, terminated in recovery; yet, I have known others in which the event has been less fortunate. A lady during her first labour was seized with puerperal convulsions, a very large bleeding and the extraction of the child removed them, but at the end of a few days she became maniacal. From the generally favourable result of these cases no apprehensions were expressed about her safety, but she died three days after the attack. The body was carefully examined by a very eminent anatomist, but no vestige of disease was discovered either in the brain or elsewhere;

where ; this shows contrary to what has been said that it may destroy life independent of inflammation. I knew another lady who had been confined three months, when she was seized with mania which after several weeks terminated fatally; the body was not examined. Some years ago I met two able and eminent physicians in consultation on a case of this kind ; one had just lost a case in a lying-in institution; the other had unexpectedly met with a similar circumstance in private practice, and was resolved in future to give a very guarded prognosis. The extensive experience of the Hospital de la Salpetriere, represents it as a much more formidable disease: out of ninety-two cases, six (that is one in fifteen) died. Those who consider puerperal mania as a disease ultimately harmless to life and understanding, will be surprised at the proportion of incurables and deaths

deaths in this hospital: the only way to explain it is by supposing that none but those cases which resist the ordinary treatment at home, that is, the picked bad cases, find their way to this institution.

Some women are so liable to insanity in the puerperal state, that they have been deranged in many successive confinements; this however is far from being always the case; I have known several who having been affected with mania after lying-in in the country, have in their next confinements come to town to be attended, they were long and carefully guarded from noise and agitation, dieted cautiously, purged frequently, and although apprehensions were long entertained about them, completely escaped their former attacks.

It has been said that mania is liable to be confounded with phrenitis, but that they might be distinguished by fever, furious delirium, and intolerance

tolerance of light, symptoms which are present in the latter, but not in the former. I confess that I place little reliance on this distinction; puerperal mania is often attended by fever, the maniacal violence is often sufficient to be termed furious, and as to intolerance of light it is not easy to ascertain it in so disordered and irritable a state of mind; if every patient who has fever, is furious, and shrinks from a candle, is judged to labour under phrenitis, mania will often be mistaken for it, and what is worse, mistreated*. These diseases would be more easily distinguished by attending to the history of the case and the succession of symptoms. In those inflammatory affections of the brain which I have observed in child-bed

* The pulse becomes extremely quick, the general heat of the body is increased, and there are in most cases the common symptoms of fever, though mania has been defined a delirium without fever (Denman, vol. 2. p. 513.)

the patient has had fever, pain, and throbbing in the head, vertigo, and at most confusion of mind, but was able and willing to describe her feelings, and this state has preceded for some time any thing like complete alienation of mind; on the contrary in mania an irritable, incoherent mind was among the first symptoms of the disease.

As to the causes of this disease *the chief seems to be that peculiar state of the sexual system, which occurs after delivery*; in many instances the only remarkable circumstance about the patient is that she has been delivered, or is nursing; yet there is another cause which greatly contributes to the excitement of this disease, a considerable interruption to that mental tranquillity so requisite during the susceptibility of the puerperal state; the frequent admission of boisterous persons into the lying-in room, an officious, eager, irritable nurse,

nurse, or relative, who with the best possible intentions is continually doing the worst possible things, sudden and violent agitation, domestic anxieties, and misfortunes have preceded, and apparently contributed to the appearance of the disease. Still patients often lie-in, are disturbed, irritated, frightened, depressed, and shocked, without becoming insane: hence we must take into the account, at least as a predisposing cause, a peculiar susceptibility of the brain.

In the treatment of puerperal insanity, all the benefit I have ever witnessed has depended on attention to the following objects. First, to protect the patient from injuring herself. Secondly, to evacuate by occasional purgatives, those impurities which in this irritated state of the nervous system are formed in the alimentary canal, and which keep up or aggravate the original disorder. Thirdly, to watch the state of the circulation, and if congestion

congestion or inflammation in the brain should supervene to remove it by antiphlogistic remedies. Fourthly, to procure sleep at night. Fifthly, to manage the mind of the patient according to circumstances, soothing it during irritation, encouraging it during depression, but above all things never to attempt the removal of her delusions by argument, and when the violence of the disease has subsided, to facilitate the recovery of her natural feelings and faculties by presenting their natural objects. On each of these subjects I have a few remarks to make.

I. Whenever it is possible the patient should be confided to a nurse accustomed to the task; she is not equally safe under the care of any other however intelligent and discreet; these nurses learn by experience all the arts which such patients employ for the attainment of their object, and regularly and methodically secure the knife, garter, door, window, &c.

There

There is no contrivance to which the patient can resort which they have not learnt by practice to anticipate; it is a striking example of the superiority of an ordinary mind, disciplined, over a fertile but undisciplined cunning.

II. The tongue is commonly more or less furred, the excretions from the bowels unhealthy either in colour, or smell, or both, the disease (whether mania or melancholia) becomes aggravated even by a tendency to constipation, and is sensibly relieved by a purgative which operates freely. Hence an important part of the treatment consists in employing occasional purgatives, and experience shows that they require to be given in doses which will operate copiously twice or thrice, and to be repeated every other or every third day; most of the good I have seen from drugs, has been effected by purgatives, though as with other diseases it has been far less striking

striking in some instances than in others.

Mrs. H. N ———, twenty-six years of age, was confined with her first child at her country seat in ——— but a few days afterwards was seized with mania ; her illness was alarming and long, but at length she recovered, and becoming pregnant again came to London to be attended by me ; she had a short and easy labour, nursed her child without difficulty, and for ten days continued free from any symptoms of disease. At this time a fire broke out not many doors off ; it was at first carefully concealed from her, but in the evening as she was standing at the window of her bed-room which looked into a yard at the back of the house, a piece of something on fire fell down before her ; this agitated her greatly, and when I saw her a few hours afterwards her pulse was quick and her talk hurried and unnatural. I slept
in

in the house ; about four in the morning she sent for me and told me she suspected she was the Virgin Mary, for there was a bright light shining about her temples ; the next day she was so violent that it became necessary to put her under the care of an experienced nurse, and occasionally to confine her with a straight waistcoat. Her nights were very sleepless ; I scarcely ever found her pulse 100, and it was soft ; her face was pale and sallow, and her eye yellowish, her tongue was furred and her bowels scarcely ever acted without medicine : her head was shaved and cupping glasses were applied to the scalp so as to draw ten ounces of blood, without any manifest benefit, she took an occasional purgative at first of salts and senna every other day, latterly of scammony every third day, and about three weeks from the commencement of the illness returned to the country well.

It

It was little more than a year afterwards when she informed me that she should soon require my attendance again, and desired me to visit her in the country as she was somewhat out of health ; she was within a week of her confinement, and as soon as I entered the room I saw that she was jaundiced ; the neighbouring surgeon told me that the same was the case in her first confinement when he had attended her ; I remembered the yellowness of her complexion and eye during her illness in town, but it was now much deeper ; the tongue was furred, the stools nearly black, the urine high-coloured. This was a state which it was desirable to correct for its own sake, but still more when the influence which hepatic disorders have over the mind, even independent of the susceptibility of the puerperal state, was taken into the account, and it was at least a striking coincidence that her former attacks had been preceded

ceded by the same appearances ; pills were ordered each containing one grain of calomel and five grains of the myrrh, and aloetic pill, and she was ordered to take one often enough to move the bowels three times daily ; one a day was sufficient, her tongue became clean, she lost the yellowness of her eye and complexion, the stools assumed a natural appearance, and she fell in labour about nine days afterwards with her stomach and bowels in a natural condition ; her friends and the attendants who had witnessed her former illnesses were long before they lost their apprehensions, but this time she passed through her confinement without the slightest appearance of her former disease.

Of course this case will produce much less effect on the mind of the reader than it did on mine, and it may be excusable to doubt whether the treatment was the cause of the escape, but in the following case

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there

there can I think be no question that the remedies employed were the cause of the striking relief which was experienced.

A lady twenty-two years of age, clever, susceptible, and given to books, was confined with her first child at ———, ——— miles from town; she was anxious to nurse it, but several days passing with little appearance of milk, doubts began to be entertained whether she would be able; *she* thought that she would; her nurse and surgeon thought that she would not: this led to irritating discussions, her manner became sharp, quick, and unnatural, and at the end of a few days she was decidedly maniacal; I now saw her in consultation with another physician; she was confined in a waistcoat and was incessantly talking, or reciting poetry; her skin was hot, her pulse full and much above 100; her tongue covered with a dark, thick fur; her
bowels

bowels were confined, and her stools excessively dark and offensive; she took a dose of calomel and jalap, followed by small doses of sulphate of magnesia; these produced a few evacuations, but they were followed by no relief; she talked almost incessantly, scarcely ever slept, and was so violent it was impossible to keep her in bed without a waistcoat. Thus three days passed from our first consultation; as the purgative had operated very moderately, and the tongue and stools were as unnatural as at first, it was thought proper in consultation with Dr. Sutherland, to procure more copious evacuations from the bowels; the next morning therefore she took a strong dose of senna and salts, made still more active by the addition of tincture of jalap; after this had been taken about three hours it procured a very large evacuation, nearly black, and horribly offensive; this was as

usual discharged into the bed without any notice on the part of the patient; it acted again an hour or two afterwards, but now the nurse who was sitting by the bed-side, was surprised to see her turn round, and in a calm and natural manner request to be taken up as her medicine was going to operate, her waistcoat was immediately loosened and she was taken out of bed, when she voided a stool of prodigious size, as dark and offensive as the first, and then walked back to her bed calm and collected; we saw her not many hours afterwards; her waistcoat was off, she was lying on her sofa perfectly tranquil, answered questions correctly, manifested no vestige of her complaint, excepting some strangeness in the expression of her countenance, and a timidity and abstinence from conversation which was not natural to her, and recovered rapidly and uninterruptedly.

III. The

III. The most difficult question in the treatment of this disease is when to draw blood, and when to abstain from it. This is certain, that blood-letting can never relieve maniacal violence except when it depends on an unnatural fulness or force in the circulation of the brain. This state of the mind therefore can never be an indication to blood-letting unless it is attended by other symptoms indicating this state of the circulation. The result of my experience is, that when there are no signs of determination to the head, and the pulse deviates from its natural state only in rapidity, (a circumstance which most probably depends on mental irritation) blood-letting, whether local or general, does not relieve the symptoms, and by diminishing strength renders the patient less capable of enduring the exhausting influence of a protracted disease. When the pulse is not only quick, but full and strong, the mental

irritation is generally abated by inducing a more tranquil state of circulation, and this, if not effected by more liberal purging and cold applications to the head, may be attained by a moderate loss of blood from the scalp or neck, by leeches, or cupping glasses; but the chief reasons for blood-letting are symptoms of determination to this organ. If the face is flushed, the conjunctiva red, the exterior vessels of the head unusually full and throbbing, the symptoms are visibly abated by shaving the head, withdrawing 8 or 12 ounces of blood, by cupping from the nucha, or scalp, applying evaporating lotions, and more frequent and active purging. The most experienced observers with whom I have had intercourse, have come to the conclusion that blood-letting is seldom necessary, or safe. Denman too speaks strongly against it, and Mr. Nesse Hill says that puerperal insanity is almost always asthenic.

IV. One of the most distressing symptoms both in mania and melancholia is want of sleep; the inefficacy of opium even in large doses in these cases has been decided by experience. I know of no means which do not frequently disappoint the intentions of the practitioner, but the most useful with which I am acquainted is the tepid bath just before bed-time, and a night dose of camphor and hyoscyamus in large doses, (10 grains of each.) When I visited the Quakers' Lunatic Asylum at York a few years ago, I was told that a supper of meat and porter was often used as a means of procuring sleep.

V. During the height of the disease, the best moral treatment will have little apparent effect, but there is a stage of the malady at which I have reason to suspect greater benefit may occasionally arise from it. My meaning, as well as the ground of my suspicion, will be best explained by

by the following case, for the accuracy of which I vouch, and with which I shall close the practical part of this paper.

A lady, twenty-eight years of age, of good constitution, but susceptible mind, became affected with melancholia a few months after her second lying-in: toward the end of her pregnancy a frightful incident had occurred to a near relation, which affected her so deeply that she often spent the night sleepless, sitting up in bed, thinking of her misfortune, and dreading that she should lose her reason after her confinement. Having nursed her child without feeding it for three or four months, with much unnecessary anxiety and exertion, she grew thin and weak, complained of sinking at the stomach and aking in the legs, and experienced so much confusion of mind that she could not arrange her domestic accounts; she became low-spirited, she

knew not why: she was advised to wean her child, took some light tonic and gentle laxative, and went down to the sea-side, but at the end of a month she returned home, having derived little benefit from her absence; her spirits became gradually more depressed, and it was impossible to persuade her she had not some fatal disease; one day it was cancer, another, inflammation in the bowels, and to such a height did her apprehensions rise, that her husband was often brought home by some alarming message, and found her with a solemn air and in a low whisper, giving directions to her servants whom she had assembled round her, what to say if she should expire before their master arrived; she now grew much worse, there was no longer any doubt about the nature of her complaint, she was seen by a physician of extensive experience in these diseases, and sent into the country;
many

many weeks passed; sometimes she was better, sometimes worse, now accusing herself of the deepest depravity, and meditating schemes of self-destruction, then again, convinced of the absurdity of her notions and struggling against the load which for a short time every day weighed on her heart; in this way many weeks passed; at length the disease came upon her with more violence than ever, and in her self-examination and condemnation she became quite ferocious.

She was now put under the care of an experienced attendant, separated entirely from her husband, children and friends, placed in a neat cottage surrounded by agreeable country (it was the finest season of the year), and visited regularly by her physician.

For several weeks she manifested no improvement: sometimes she was occupied with one notion, sometimes
with

with another, but they were always of the most gloomy description; at length it became her firm belief that she was to be executed for her crimes in the most public and disgraceful way; every noise she heard was that of the workmen erecting the scaffold, every carriage the officers of justice assembling at the execution; but what affected her most deeply was that her infamy had occasioned the disgrace and death of her children and husband, and that his spirit haunted her. As soon as the evening closed she would station herself at a window at the back of the cottage, and fix her eyes on a white post that could be seen through the dusk; this was the ghost of her husband; day and night he was whistling in her ears.

Several weeks passed in this way; the daily reports varied but announced nothing happy; at length her husband became impatient and begged to have an interview with her, thinking that
that

that the best way to convince her he was not dead was to shew himself; this was objected to; he was told the general fact that patients are more likely to recover when completely separated from their friends, that if she saw him she would say it was not himself but his ghost, but the husband was obstinate and an interview was consented to. When he arrived at the cottage he was told that she had had a tolerable night, was rather more tranquil, but that there was no abatement of her gloomy notions. "As soon as I entered the drawing-room, where she usually spent the day (I copy his own statement which I have now before me, and which he wrote down at the time of the occurrence), she ran into a corner, hid her face in her handkerchief, then turned round, looked me in the face, one moment appearing delighted at the thought that I was alive, but immediately afterwards assuming a hideous

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ous expression of countenance, and screaming out that I was dead and come to haunt her. This was exactly what Dr. —— had anticipated, and for some minutes I thought all was lost.”

“ Finding that persuasion, and argument only irritated and confirmed her in her belief I desisted, and tried to draw off her attention to other subjects ; it was some time since she had seen either me or her children ; I put her arm under mine, took her into the garden, and began to relate what had occurred to me and them since we parted ; this excited her attention, she soon became interested, and I entered with the utmost minuteness and circumstantiality into the affairs of the nursery, her home, and her friends. I now felt that I was gaining ground, and when I thought I had complete possession of her mind, I ventured to ask her in a joking manner whether I was not
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very communicative for a ghost ; she laughed ; I immediately drew her from the subject, and again engaged her attention with her children and friends. The plan succeeded beyond my hope ; I dined, spent the evening with her, and left her at night perfectly herself again."

He went the next morning, in a state of intense anxiety to know whether his success had been permanent, but her appearance at the window with a cheerful countenance soon relieved his apprehensions : while he was there Dr. ——— came in ; he went up stairs without knowing the effect of the interview, and came down, saying, " it looks like magic ! " With a view of confirming her recovery she was ordered to the sea side to bathe ; as soon as the day of her departure was fixed, she began to droop again, the evening before it, was very low, and on the morning of her setting off was as bad as ever ; this state continued

continued for several weeks, in spite of sea air and bathing, and ceased as suddenly as it had done before, apparently in consequence of interviews with friends, calculated to remove the apprehensions by which her mind was haunted. She has since then continued perfectly well, and has had another child without the slightest threatening of her former malady.

The conclusion which I deduce from the foregoing case is, not that violent mania is curable by conversation, (if it should occasion the irruption of relatives during the heighth of the disease, the communication may do more harm than good) but that there is a stage approaching convalescence, in which the bodily disease is loosening its hold over the mental faculties, and in which the latter are capable of being drawn out of the former by judicious appeals to the mind.

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I know too well that striking cases give an inaccurate notion of the average influence of remedies to expect that similar conduct will frequently be followed by similar success, yet I would ask these questions: How long would this patient have remained in a disordered state of mind, if she had not been treated in this way? and again, how many persons are there at this time in a similar state who (although those who have the care of them do not suspect it) are capable of being restored in a similar way to their natural views and feelings?

Such are the most important particulars about a malady which, though seldom either fatal or lasting, is one of the most terrifying that occurs during child-bed or nursing: they constitute, it seems to me, a strong argument against the prevailing doctrine that insanity is a moral disease, that is, a disease of the mind independent
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of the body, and as this doctrine is likely to have a practical influence on our future investigations into the pathology of this disease, I shall venture to explain what has occurred to me on the subject.

What are the grounds of the prevailing doctrine, that mental derangement is a moral disease?

IT is not only the popular belief but a notion prevalent among medical men, that insanity is a disease not in our physical but our moral constitution. This is not a speculation merely, but leads to various practical conclusions: that it is a subject rather for metaphysical than for medical inquiry; that a physician is requisite for a mad-house only as he is for a school or prison; that the true theory of these mysterious diseases will be found in some extraordinary

ordinary obliquities of thought, the true cure in some moral regimen, some training system for the weak, or crooked faculties of the understanding. A few medical writers have taken a different view of the subject, supposing the derangement of the mind to depend on a morbid condition of the body, but as they have rather assumed than proved the proposition in question, it may be useful to inquire into the reasons which have led to this theory, and into the degree of reliance which ought to be placed upon them.

Let it be remembered then, that there are many diseases in which some of the faculties of the mind in a certain degree deviate from their natural state: such are the incubus or nightmare, severe and habitual indigestion, so often attended by lassitude of mind and depression of spirits, the hypochondriasis of liver disease, the strange and different forms of hysteria, and lastly, but most remarkably fever attended

tended by delirium : no one supposes these to be moral diseases ; no doubt is entertained that the mind is affected by disease of the body ; the mental symptoms are universally considered as the natural effects and signs of disturbance in the brain ; a man of plain sense therefore, familiarly acquainted with these facts, would naturally look upon insanity in the same light, unless some solid reasons can be given him to the contrary ; where are such reasons to be found ?

I. One cause of the belief that insanity is a moral disease is, that it is often produced by passions, or efforts of mind ; as the cause and the ultimate effect are both mental, it has been thought that the disease must necessarily be a moral one. A very little observation however will teach us that no causes operate more distinctly upon our bodily organs than violent efforts and agitations of mind ; will not terror so enfeeble the muscles as

to make us as tired as after a toilsome walk? will not the anxiety of a few hours deprive us of appetite for the next meal? will not long continued grief or intense study so impair the process of nutrition as to emaciate the body and bleach the cheek? will not mental agitation set the heart beating, the pulse throbbing, and crimson the countenance? Those states of mind which are capable of producing insanity, act distinctly upon the bodily organs, and if they are capable of disturbing the muscles, the stomach, the organs of nutrition, the heart and blood-vessels even to the little blood-vessels of the cheek with which the mind is only secondarily connected, where is the difficulty of supposing that they may act physically upon the brain, with which it is immediately connected; if mental effort or agitation induces physical disturbance in the brain, this physical disturbance will shew
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itself by an unnatural state of the mind; thus the cause may be mental and the ultimate effect mental, yet the intermediate process itself may be essentially a physical one.

II. Another cause for the moral theory of insanity is, the supposition that the bodily disease which accompanies it is too slight, to occasion the mental derangement; but this will no more bear examination, than the former. For first, the bodily disease which accompanies insanity may not be so striking as that which accompanies delirium; it may not be so noticeable as a hot skin, a quick pulse, a furred tongue, and helplessness of limbs, yet it may be equally, or even more, capable of disturbing the actions of the brain. A little black bile which none would notice but an explorer of hepatic secretions, may be more capable of disordering the faculties of the mind than loss of appetite and prostration of strength,

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which every one would notice. It is certain that puerperal insanity depends on a peculiar state of the bodily constitution, yet this state, so far from being obvious, is often known to exist only by a disordered condition of the mind. This objection is founded on the supposition that bodily disease is only capable of disturbing the brain of the patient in the same degree that it strikes the senses of an observer, which is a manifest absurdity. Besides, in the delirium of fever we consider that the deranged state of the mind, is the effect of a disordered state of the brain; why then should not the same symptoms prove the same thing though unconnected with the same extent of disease; insanity, is itself a proof of brain, that is, of bodily disease.

III. It is well known that strange habits of mind, long continued, are capable of generating great singularity of opinion and feeling; between this
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moral eccentricity and insanity there is sometimes a striking resemblance. It is not easy to confound boisterous madness with healthy singularity, but when a lunatic is harmless in conduct, insane only on one point, and talks so rationally on all others that it is not easy to detect his infirmity, his state of mind is wonderfully like those eccentric and absurd opinions which intellectual habits are capable of producing, and which often cause their possessors to be called mad, half in joke yet half in earnest; a little insight into the mode of their production will enable us to judge whether this resemblance be apparent or real.

It is so well known that the mind may brood over a subject till it loses the power of seeing it in a right point of view, that it is commonly said a man may tell a lie till he believes it. "I wish" (says Dr. Johnson, rebuking Boswell for

for the zeal into which he had worked himself about the history of Corsica,) “ I wish there were some cure like the lover’s leap for all heads of which some single idea has obtained an unreasonable and irregular possession.” Objects which have had frequent access to the mind seem to have a double power over it; viz. they not only produce the natural effect of a single application, but they revive the traces, or recollections of their former impressions. This is the case not only with objects of fancy, but with propositions which appeal to the understanding; an opinion produces effect partly in proportion to the manifest truth which it contains, and partly to the frequency with which it has been so presented to the mind as to excite the feeling of approval or conviction, every time it is so applied it leaves (if I may so express myself) a stratum of influence in the mind: this is capable of incalculable accumulation

mulation till at length the object produces an effect, and gains a power over the individual totally different to what it possesses over one less frequently impressed by it. Objects by repetition lose their power over the senses, for the senses have no memory, while they incalculably augment it over the understanding and the affections. It is on this principle that so many trifles acquire an influence over us so disproportionate to their importance; that with the generality of mankind opinions owe their power more to habit than to evidence; that an old song however bad pleases more than a new one however good; that a wag tickles those who are accustomed to him more than those who are not; that the ploughman prefers his coarse and awkward mistress, to the loveliest lady in the land; thus the constant dropping of little circumstances on the character wear
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in it deeper channels, than those of greater power but rarer occurrence.

Another effect which intellectual habits produce in sane minds, is to alter the natural proportion between the liveliness of our sensations and the indistinctness of our thoughts.

“Studious men waste many an hour sitting alone and musing passively on the passing ideas, when the mind as it were lies on its oars, and floats gently down the stream of thought, with only now and then a stroke to guide it or propel it. Every man has his forms of idleness, and this is the harmless idleness of contemplative minds.” It is an idleness however which has a material influence over the character; we distinguish outward objects from inward thoughts by the greater distinctness of the former; but the distinctness of any impression may be increased or diminished according to the degree of attention directed

directed to it. He therefore who habitually directs his attention more to his internal thoughts, and less to external objects, than most persons, will find himself in time differing from his neighbours in the power of doing it; in the vividness of his ideas and the obtuseness of his sensations. This is the true theory of absence, reveries. In dreams we mistake our thoughts for realities, because in sleep which is the absence of external sense, they are no longer extinguished by the superior brightness of external objects. It is possible therefore to conceive such a diminution in the distinctness of our sensations, and such an increase in the distinctness of our thoughts brought about by habit in a sane mind, that the one shall be mistaken for the other in a kind of day-dream. These are some of the distortions which intellect contracts by long seclusion. The mind as well as the body grows diseased by confinement,

ment, and requires to go abroad and take exercise and breathe the fresh air of the outward world.

It seems then that habits of mind, processes purely intellectual produce two effects, very similar to the two main features of mental derangement; a conviction disproportionate to the degree of evidence, and a diminution of that inequality between the strength of our outward and our inward impressions, by which we are protected from mistaking the one for the other. How far this is capable of being carried, and how near these effects may approach to a perfect resemblance with insanity, it may be difficult to say. The secret history of cloisters and hermitages would supply curious facts towards a solution of this question. (See the life of St. Theresa?)

Be this as it may, it is this striking similarity between the erroneous opinions of the insane, and the singular opinions

opinions of the excentric, and this power of habit to generate even in healthy minds, something so similar to the essential features of insanity, which has been I suspect one of the principal causes for the belief in the moral nature of this disease. Yet any one who is familiar with human nature both sane and insane would perceive an important difference between the two cases; it is this, that the errors of the excentric are the result of long habits, continued for a great part of our lives, and fabricated by slow and almost imperceptible degrees, while the errors of the insane spring up suddenly, within a few months or even weeks. The patient has suffered some mental agitation, received a blow on the head, has been lying-in, or is recovering from a fever; the mind becomes confused and hurried, and in a few weeks or even days there arise the wildest and most absurd beliefs. In these cases there is neither time nor peculiarity

liarity of habits adequate to explain such effects by the intellectual processes above alluded to. Between the erroneous opinions of the insane, and the singular opinions of the excentric, there is the same difference as there is between that permanent readiness of argument, imagery and language, which is the result of study and practice, and those sudden and temporary gusts of eloquence produced by a bottle of wine.

IV. It is possible that the reader may be satisfied about the foregoing points, yet there remains another consideration which may induce him to believe that insanity is at least sometimes a moral disease ; the consideration is this, that the cure of insanity is sometimes effected by a method both moral in its nature and moral in its operation ; thus, patients have been relieved from their hallucinations, and restored at once to complete sanity by a well contrived incident,

dent, a well expressed argument, or a well managed conversation. Here the cure is as much an intellectual process as the conviction of a sophist, or the conversion of an infidel ; in fact they are identical processes. Now I would first remind the reader of the general truth of the opposite statement, the total inefficacy of argument, persuasion, and all appeals to the mind during the violence of the disease ; but granting it to the full extent of the truth, do we not witness the same effects brought about by the same means in affections whose origin is confessedly physical ; if a person low spirited from ill health should suddenly receive some joyous news, he would for a time at least be restored to the hilarity of health ; would forget his low spirits in his bright prospects ; the physical would yield to the mental impulse. A more striking example sometimes occurs in delirium ; a person in the height of fever
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who if left to himself would stare vacantly and talk incoherently ; will sometimes at a loud question, wake up, answer collectedly, be himself for a short time, and then relapse into his former incoherence. Here is a mental cause removing by a mental operation a state of mind, avowedly physical in its origin. From such facts what then are we to infer ? not that insanity is a moral disease because it is sometimes benefitted by moral treatment, but that though the unnatural state of the mind arises from a diseased condition of the body, there are times and states in which the mind begins to regain the capability of being acted upon in the natural way by its natural, mental objects.

There is another circumstance which cannot be numbered among the *reasons* for the moral theory of insanity, but which I suspect has greatly contributed to it ; I mean the
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fear that the opposite notion would favour the doctrine of the materiality of the soul. A moment's reflection however will shew that the physical theory of insanity is not more likely to lead to this conclusion, than the physical theory of delirium which no one doubts. If the effect of bodily disease on the mental faculties is likely to have a dangerous influence, the danger is not in this or that instance, but in the principle. As the present question however is, not what is the safest doctrine, but what is the truest; as the example of insanity adds nothing to the danger of the principle, and as this is not the place for discussing the question how far it is compatible with the immortality of the soul, I shall not enter into it; I may be allowed however to remark that the whole danger rests on this proposition, that if the diseases of the body disorder the faculties of the mind, then the faculties of the mind must

must be the functions of the body. But where is the proof of this? disease in the liver impairs digestion; is digestion a function of the liver? defects in the cornea confuse the perceptions of the retina; is vision a function of the cornea? it is plain therefore that disease in a part may disorder actions which are not the functions of that part. The mind is affected by the state of the stomach, of the liver, of the uterus; we know therefore that it may be affected by the state of organs with which it communicates, but of which no one pretends it is the function. It may be said that the mind and these organs can never be seen apart, and that although it is not supposed to belong to them in the same way as a function belongs to an organ, we have no proof that they can exist separately; true, but take another instance. A hand shattered by the bursting of a gun will produce fever and delirium; let

let the limb be removed at the proper period, the constitution regains its tranquillity, and the mind its powers: it is clear therefore that the mind may be affected by disease in a part not only of which it is not the function, but from which it may be separated; that such is the construction of a living body, that one thing may affect another with which it has only a temporary connection; where then is the danger of the principle that the diseases of the body disorder the faculties of the mind? Whoever has convinced himself by other considerations, of the immortality of the soul, will find no difficulty in this principle, and so far from shrinking from it will rather see in it a reason for believing that in a separate state of existence, “it is highly probable that the soul works clearer, and understands brighter, and discourses wiser, and rejoices louder, and loves nobler, and

desires purer, and hopes stronger than it can do here." (Jeremy Taylor's Sermon on the death of the Countess of Carberry.)

It appears therefore that emotions of mind are capable of disturbing the organs of the body, and that though moral causes in themselves they may be physical in their operation; that the adequacy of bodily disease to disorder the mind of the patient is not to be estimated by the degree in which it strikes the attention of the observer; that although the erroneous opinions of the insane are very similar to the singular opinions of the eccentric, they are very different in their nature and origin; that causes moral both in their nature and operation, are capable of influencing diseases which are avowedly physical, and that consequently their influence in insanity is no proof that it is a moral disease; lastly, that the physical theory of insanity is no more a proof of

of materialism than many avowed instances of the influence of body over mind. I conclude, therefore, that there is no ground for the reasons which have led to the belief in the moral nature of insanity; if we take into the account the influence of physical causes in its production, as injuries of the head, parturition, drunkenness, the sun's heat, and the influence of medicinal remedies in abating or removing it, can we avoid taking it from the solitary and singular station which it holds as a moral affection, and replacing it among those in which an unnatural state of mind attends on bodily disease?

If this was merely a speculative question, an inactive scepticism might be philosophical and justifiable; but it is one of the many we meet with in life which cannot be answered with mathematical certainty, but which should be settled as well as we are able, because they are necessary for

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action.

action. It is no less a question than whether in our search after a better theory and a more successful treatment of these diseases, we shall occupy ourselves in investigating the causes and treatment of disease in the brain, or in discussing, whether insanity is an error of the perception, the imagination, or the judgment; in applying the inductive mode of reasoning to its intellectual phænomena? who are the persons best fitted for prosecuting the inquiry? those who are familiarly acquainted with the causes, progress, and treatment of diseases in general, and among them of the diseases of the brain, or those whose only qualifications for the task are their benevolence, their zeal, and their metaphysics?

XIII. *Some Observations on the Duodenum, with Plates descriptive of its situation and connections. Extracted from the Gulstonian Lectures. By G. D. YEATS, M. D. F. R. S. Fellow of the Royal College of Physicians, &c. &c.*

Read at the COLLEGE, in May 1817.

WHOEVER examines with accuracy and attention the situation, structure and connections of the intestinal canal, must be immediately struck with the appearances in these different respects of the duodenum. In the various systems of physic which have been published, the diseases to which this intestine is likely to be and is in fact often subject, are not at all noticed. A detached essay or two have in former periods been published; one valuable one by Hoffman*, replete with many excel-

* In the sixth volume of his works published at Geneva, 1740.

lent remarks, and another by Dr. Claussen in 1757*.

Previous to the publication of Claussen, but subsequent to that of Hoffman, Dr. Monro published a description and an account of the uses of the intestinum duodenum†; the earliest account I have read, and an accurate one it is though very concise, is that which was published by Bonnazoli in 1732‡. He seemed very sensible of the importance of this intestine from the remarks he has made. Notwithstanding these several treatises however, an account of the diseases of this intestine is still a desideratum. Bonnazoli indeed and Hoffman and Monro and Claussen have

* Republished in a collection of Medical Dissertations by Dr. Sandifort, in the third volume of his Thesaurus in 1778.

† In 1752, in the fourth volume of the Edinburgh Medical Essays.

‡ Republished in the second volume of the Transactions of the Bologna Academy in 1745.

given us some very valuable observations, both practical and anatomical, but the path they have laid open has been untrodden; I believe therefore in a practical point of view both its functions and diseases have been very much overlooked, insomuch that diseases have been referred to other organs particularly the liver, and have been treated as such although they have most certainly taken their origin from a morbid condition of the duodenum itself.

It will better illustrate its nosology if a few observations respecting its functions and uses are premised.

After the food in the stomach is reduced into a pultaceous mass, it is past into the duodenum very much lessened in bulk both by the liquid absorption and the diminished state of aggregation of the solid parts, in consequence of being broken down and digested by the agency of the gastric solvent; the functions of the
duodenum

duodenum now commence. As soon as the chyme has past the pylorus, it becomes mixed with the liquid poured from innumerable glands more particularly situated at the commencement of the duodenum ; and from the tortuous tract through which the chyme has to travel, it is intimately blended with this liquid and becomes in fact perfectly churned by the agitations it has undergone, so that, I believe, it becomes in qualities a different mass to what it was when it first passed the pylorus, and before the chyle is separated from it.

Being propelled by the action of the intestine and in part by that of the diaphragm during respiration, the chyme is brought to the sacculated angle of the duodenum where it is attached to the capsule of the right kidney. In this most depending part of the intestine, the chyme must be delayed for a time from its situation, and here meeting with the pancreatic
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and biliary liquids, it is not only intimately mixed with them, but undergoes a change by which is produced the chyle.

And here perhaps it is deserving of remark, that the agitation of the contents of the duodenum, by which their several parts become more intimately united, not only takes place from the action of the duodenum itself, but as it is tied to the colon, whatever action takes place in this intestine, must more or less be communicated to the other; a circumstance which I believe has been overlooked, but upon which I am disposed to lay some stress. In this way it is probable a great part of the evil arises, which takes place in constipation; for when the colon is much distended with hardened fæces, it must press with some force on the ascending part of the duodenum, thus still further impeding the progress of the contents of the latter which have also to rise
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against gravity, when the body is in an erect position or recumbent on the right side. For this reason, it is always adviseable, previous to giving an opiate glyster where there is violent pain and vomiting from a gall-stone sticking in the duct, especially if the bowels have not been recently evacuated, to direct an opening enema to be injected, to unload the colon of those contents which, by distending it, cause pressure on that very part of the duodenum where the ductus communis opens, and thus impede the passage of the stone into the intestine. Be it recollected too that this bend of the duodenum is exactly between the right kidney and the ascending part of the colon, and that therefore much distention of it would in fact force the duodenum against the hard substance of the kidney, and would contribute to obliterate its cavity by the pressure of its sides against each other. The contents of
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the duodenum being properly mixed and digested in this bend, and the chyle prepared by a process with which we are totally unacquainted, are propelled through its ascending part into the jejunum which begins where the duodenum makes its exit from the ring in the mesentery ; (H. Plate I.) During the transmission of these contents a considerable absorption takes place, for this intestine is thick set with lacteals ; and this absorption occurs before the complete formation of the chyle, for turgid lacteals have been seen passing in great numbers from its commencement in its course under the liver*. Thus then the duodenum is an organ of considerable importance in its functions, and that the Divine Author of our Being intended it for pre-eminent

* I have seen the lacteals full of white chyle in the *very beginning* of the duodenum, and through its whole length. Cruikshank's Anatomy of the Absorbing Vessels, page 149.

uses is evident, I think, also from the peculiar manner in which it is protected. It is more covered and guarded than any one of the intestines, and indeed than almost any viscus of the abdomen; it lies concealed deep under the liver which forms an anterior protection to it, all the muscles of the back, the spine, the ribs and the right kidney protect it behind; again when it emerges from its concealment, it takes the protection of the colon which lies over it: it escapes our sight again by dipping under the soft bed of the mesentery, and is, moreover, nearly throughout its whole extent, covered with the mesocolon and with a considerable quantity of fat and cellular membrane, which lies between the lamina of the mesocolon and peritonæum. All the other intestines with the stomach may be called floating viscera, this on the contrary is securely tied to the liver, right kidney, colon and back: by its envelopement

lopiement in the mesentery its extremity is kept more steady where it passes through the ring ; it is studded with innumerable glands for the secretion of its own peculiar fluid ; it is extremely vascular and the nerves which belong to it have an extensive communication with the rest of the body by a connection with the par vagum and great sympathetic, through the medium of the semilunar ganglions. Its internal coat is thrown into numerous folds like the stomach, and there are poured into it very important fluids from the pancreas, gall-bladder, and liver. Very wise purposes too are answered by the manner in which the duodenum is tied down and connected. Had it descended immediately from the pylorus without being attached to the liver by the omentum minus, it would not only have drawn down the small end of the stomach out of its proper place, but when the stomach contained food there would be always
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danger of its passing into the duodenum whenever the pylorus was relaxed ; as it is, the food can for certainty remain the necessary time in the stomach for solution. Had the duodenum too been a loose and floating viscus like the other intestines, it would frequently become entangled with the pancreatic and biliary ducts, thus impeding its own action and also the transmission of the necessary fluids through these ducts, and moreover the food would pass quickly through it in its pendulous state instead of being detained the proper length of time to undergo the change into chyle. It is the most important recipient in the human body, receiving more various kinds of ingesta than any other organ : and notwithstanding it is less than the stomach, it has a greater capacity than the other intestines except the colon. It was necessary that the stomach should not only be greater because it receives the food in the gross, if I may

so express it, but on account of the large quantity of ingesta, particularly of liquids which is sometimes thrown suddenly into it. The duodenum on the contrary receives the food gradually through the pylorus and in a small quantity at a time and in a more homogeneous state. The greater size of the colon is evidently intended for the accumulation of fæces previous to their discharge, and it may very well be considered as the excretory reservoir to the digestive organs. From this general view of the functions and connections of the duodenum, it may very justly be regarded as a second stomach in which the process of digestion is as much carried on as in the first, that it serves very important purposes in health, and that consequently it is the seat of some painful and dangerous diseases.

In considering the diseases of the duodenum, it is evident that it must be liable to all the consequences of an
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imperfect digestion in a similar manner as the stomach, with this difference that the symptoms will vary from its situation and immediate connections anatomically considered as well as from its own peculiar functions. It appears to me indeed that the consequences of a long continued indigestion in this intestine will be more distressing than in the stomach itself; for from the elaborate manner in which this intestine is constructed, from the important fluids poured into it, and from the care taken to confine it in its course, I am inclined to believe that the proper digestion in it, has the power of correcting, in a great measure, what has been improperly performed in the stomach. On the contrary should a very imperfect digestion take place in the duodenum, I do not perceive, from their anatomical structure or from their probable functions, that any improvement can occur in any subsequent part of the alimentary canal;

canal; hence it becomes a matter of the first importance to the proper nourishment of our whole frame, and to the comfort of all our feelings, that the duodenum should be in a perfectly healthy state: for from the sympathy which exists between the chylipoietic nerves and almost every part of the body, distant symptoms are produced of a very painful nature; and from the morbid condition of the duodenum, irregular peristaltic action takes place, torpor in one part, spasm in another, and perhaps a retrograde action in a third, with constipation and a morbid appearance of the fœcal discharge.

One of the most constant effects of the indigestion of our food is the extrication of a considerable quantity of gas, because then a fermentation takes place instead of the proper assimilation by the living powers. I have no doubt that under the circumstance of much flatulence gas is actually secre-

ted. From the particular situation and connection of the duodenum, more distressing effects will arise in it than in any other part of the alimentary tube. Its distension by extricated air will cause very great pain by stretching its nerves, and the irritation will be immediately propagated through these nerves to very important parts.

The duodenal nerves come off from the right hepatic plexus, which also supplies the gall-bladder, biliary ducts, pylorus, pancreas and kidneys, and then they pass on to the substance of the liver ; hence we see that duodenal distension will by nervous connection (independent of mechanical effects) produce irritation in all these parts. The intestines have their healthy peristaltic action maintained by the nervous influence from the solar plexus, through the nerves distributed to their fine and beautiful vascular coat ; an irritation of these nerves will not only

only cause spasm, but by continuance inflammatory action. The duodenum is extremely liable to the same effects: it is supplied with nerves directly from the eighth pair which communicate with the stomach and pylorus; thus besides the universal connection between the par vagum and sympathetic, it receives some filaments from the semilunar ganglions and hepatic plexus; and as at its lower end it receives nerves from the superior mesenteric plexus which communicates with the renal plexus, it is clear how a morbidly irritated duodenum will produce uneasiness in nearly the whole of the abdominal viscera, causing irritation in the intestines, stomach, œsophagus, kidneys, the right particularly, liver, &c. &c. and as the hepatic plexus is formed from the semilunar ganglions, in which are concentrated not only the nerves which supply the chylopoietic viscera, but in which terminate those nerves which, in their progress from the brain, either give branches to, or

inosculate with, nerves which supply the œsophagus, lungs, heart, diaphragm trachæa, larynx, pharynx, chest, muscles of the face, lower jaw, and neck; hence the headach, vertigo, contortions of the countenance, rolling of the eyes, asthma, cough, pains about the chest and shoulders, which accompany duodenal irritation. And that while oily emulsions and expectorants, which only add to the mischief, have been prescribed in such coughs and asthmas, medicines of a very different class should be given*.

In addition to these distant symptoms produced by nervous sympathy, painful sensations of a more local nature, arising from mechanical pressure from distension of this intestine, will take place, first great pain and spasm in itself: pressure will be caused upon the pancreatic and biliary ducts, preventing the flow of the fluids of their

* See pages 91, 92, of my Letter to Dr. Wall on Hydrencephalus, with the quotation from Hoffman in the note.

glands into the duodenum ; hence if the pressure be continued, a soreness and sense of fulness will be felt below the pit of the stomach in the situation of, but deeper seated than, the arch of the colon, a jaundiced appearance of the skin will take place with a puffiness and uneasiness on pressure just before the cartilage of the eighth rib, arising not only from the fulness of the duodenum itself, but from the distension of the biliary and pancreatic ducts. I have no doubt that under such circumstances patients have been treated for a liver disease by salivation, with a perfectly sound liver, not only without any advantage, but with positive detriment to the constitution. I have seen cases which have created this belief in my mind.

Much is said by different writers of the repletion of the duodenum by food, causing the bile to flow from the gall-bladder for the purposes of digestion ; I believe the very contrary effect takes place from its distension, and the anatomical

tomical position of the parts will shew this to be the case. (See C. C. D. and E. E. of Plate I.) The gall-bladder lies rather in a slanting direction, with its neck upon the duodenum, and the side of its fundus upon the colon; now if the duodenum press the neck against the liver, it must confine the bile in the sac instead of discharging it according to the generally received opinion, more especially when we consider that the cystic duct at its commencement goes off, ascending at right angles from the bladder. So that did the quantity of food in the duodenum cause pressure on the gall-bladder, it would prevent the flow of bile from it; but under ordinary circumstances I do not think that the food in this intestine is in such a quantity as to produce this effect: in great feeders it appears sometimes to occur; with such persons how often do we see a jaundiced appearance of the eyes, produced probably in part by the cause just mentioned. These people also
very

very often suffer from the dyspepsia crapulosa, whence will arise additional causes giving rise to this effect. Should it so happen that calculi are contained in the gall-bladder, the pressure from the distended duodenum will cause additional pain by rubbing the rough stones against its sides. A great deal of pain will be felt across the back, and especially in the region of the right kidney, from the connection which the duodenum has with its capsule, and a quantity of pale urine is often past from irritation of the renal vessels both locally and through the medium of the renal plexus of nerves. Pressure will also be made by this distension upon the lacteals and glands of the mesentery, and will interrupt the ready transmission of the chyle to the thoracic duct.

With all this distension and pressure, a good deal of interruption must be given to the supply of the blood by the arteries and its removal by the veins of
the

the intestine itself. From this interruption, obstruction and inflammatory irritation will arise ; and it is very probable the Infantile Remittent is produced in this way. The best mode of practice for the complaint, with the symptoms which take place in the protracted disease, confirms this opinion, for the food comes away perfectly undigested, which would not be the case if the duodenum were in healthy action. Other mischiefs will arise giving considerable interruption to the more general as well as to the local circulation. That part of the vena cava inferior, which lies under the duodenum, will be prest upon and will form an impediment to the returning blood, and if this pressure be often repeated and long continued, it will probably cause varicose veins in the legs.

It is not uncommon for persons, troubled with what is called flatulent distension of the stomach, to feel faint with a fluttering pulse. I can readily conceive that

that the pressure of the cava against the spine by the distended duodenum, will cut off such an immediate supply of blood to the heart as to produce this effect. The great meseraic vein too which rises above the transverse ascending portion of the duodenum, will also suffer by pressure, though in a less degree according to circumstances, and produce accumulation there and in the splenic vein, hence the pain which persons who are dyspeptic, and said to labour under liver complaints, feel in the region of the spleen. The force with which the aorta and its immediate branches beat, will probably not admit of their being much affected by the pressure ; but the resistance to the passage of the blood in the meseraic vein, will in some degree impede the transmission of it from the corresponding artery, and will assist in accounting for that throbbing frequently met with and easily felt in the epigastric region of dyspeptics.

I have

I have witnessed it so strong as to create a suspicion of aneurism.

As this morbid state continues it will affect the adjoining organs, and unhealthy discharges will take place from the liver and pancreas, and from the duodenal glands, causing a great accumulation of irritating matter in its sacculated dependent part, which the intestine will not easily propel, not only as it has to carry it against gravity in a direct ascent to its jejunal termination, but because from over distension it will be less able to contract with sufficient force; hence a torpid state takes place in it with a corresponding effect in the alimentary canal, and languor and lassitude prevail. I wish also to call the attention to another cause of difficulty in the propulsion of the duodenal contents, particularly under the circumstances above stated; namely, the confinement at its termination within the ring at the root of the mesentery which will

not admit of the diameter of the intestine being much increased at that part. At this confined point the more indigestible and fœcal portion of the food will sometimes lodge and produce a temporary plugging up of the passage, especially if the food has been glutinous, and has been rendered more solid and firm by any cause. With children, who often swallow cherry and plumstones, it is not improbable that this state does at times occur, and this cause of the retardation of the propulsion of the duodenal contents does not appear altogether theoretical. The indigestibility of nuts, and the distress in the bowels produced by eating a quantity of them, are proverbial. It is very probable that the inconvenience arises from the difficulty which the hardened indigested mass, formed into a ball with the mucus of the intestine, finds in passing this part; should it stick there inflammation will occur with vomiting,

vomiting, which, inverting the intestinal action, will bring the mass back again into the body of the duodenum, where by the afflux of more fluids from the pancreas, liver and duodenum, it will be softened and more reduced, and will thus ultimately pass, if it be not in the meantime thrown into the stomach, and rejected amidst its other contents by vomiting: here a caution presents itself respecting active purgatives in obstinate constipation, which, by irritating the intestine above the obstructed part, where the obstructing matter is too large to pass the narrowed channel, must increase the mischief. Obstructions thus frequently occurring to the passage of the duodenal contents, will of course cause distension in it above the mesenteric ring, producing all the consequences of irritation with increased acrimony of the contents by stagnation.

A repetition of this morbid condition of the duodenum, alternating
with

with partial discharges of its contents by occasional vomiting, producing only temporary relief, will sooner or later induce very serious evils, especially in children, whose appetite in such a state is often bulimious, not arising from healthy hunger but from irritation. I have no doubt that the repeated absorption by the lacteals of the acrid contents of the duodenum, produces disease and enlargement of the mesenteric glands, and ultimately *tabes mesenterica*. It is well known that the conveyance of acrid matter by the lymphatics causes enlargement, pain and inflammation of their glands; it is remarkably instanced in the knotted and enlarged appearances of the lymphatics of the sides of the neck by the absorption from the sore heads of children. In the same way diseased mesenteric glands occur in them from the acrid condition of the duodenal contents.

The

The liver, pancreas, and duodenal glands, will become diseased from congestion and irritation, and this irritation will be propagated by nervous communication to the brain; it will sooner or later run into inflammatory action and produce effusion there, or the irritation will chronically continue giving rise to spasms, convulsions, vomiting, contortions of the countenance, affections of the sight, violent headaches, faltering voice, chorea and palsy. I am inclined to believe that the disease described in Case X. of the Appendix to the Pamphlet on Water in the Brain was connected with duodenal irritation; see page 83.

Such then are the effects which digestive irritation produces in children; effects frightful in their appearances, painful and very often fatal in their consequences. I would not lay so much stress upon these circumstances,

cumstances, were I not fully convinced from observation of the great good to be derived from attending to the derangement of this intestine. In adults too I have noticed a disease of the duodenum, which from its being often overlooked, and in fact mistaken for a liver disease, claims considerable attention. The appetite gradually declines in some, in others it is very various, flatulence is troublesome, considerable languor and lassitude, accompanied by a feel as if the legs would give way, take place; occasional chills, feverish heats, pain with a sensation of weight in the right hypochondrium and across the loins, bowels torpid with dark coloured fæces, and they are often past without being figured and in small quantity twice or thrice in the day.

Urine of a mahogany colour, sometimes transparent and sometimes with a lateritious, but oftener with a white sediment, and thick throughout like gruel

gruel and water, a little nausea; nights past in a restless manner, tongue covered with a white crust, brown towards the roots; thirst is not much complained of, giddiness at times with headach, or rather an uneasiness in the head, with an unpleasant state of the sight, vision being at times obscured with motes dancing before the eyes; the pulse is not generally quickened; sometimes I have observed it to be preternaturally slow: there is frequently an intermission in it, and the patients are harassed to a very great degree with those symptoms usually termed nervous*.

Food is not stated to cause particular oppression, except that it gives the sensation of not being moved forwards so easily as formerly; in fact no complaint is made of any distress in the region of the stomach. A

* See pages 12 and 13, of the Appendix to the Pamphlet on Hydrencephalus.

dyspnœa, with sometimes a troublesome cough, adds to the restlessness at nights, the sleep of which is also disturbed by disagreeable dreams; and uneasiness is complained of over the pectoral muscles, which are sore to the touch, from which the patient imagines he is consumptive. There is a despondency of spirits, a forgetfulness takes place, subjects pass across the mind with rapidity, for the mind cannot dwell long upon any subject. After a continuance of these symptoms for a time, a yellow tinge takes place in the tunica adnata of the eyes.

Upon examining patients with the above symptoms, a fulness and puffiness will be sometimes perceived to the right of the pit of the stomach. I say sometimes, because it will depend upon the quantity of extricated gas confined in the duodenum, or upon the existence of morbid action at the time, whether this symptom be present

sent or not. By pressure *on* the region of the liver no uneasiness will be complained of, but if the pressure be made with the edge of the open hand under the ribs with the palm of it flat upon the abdomen, considerable uneasiness will be -complained of up towards the liver and down towards the right kidney, a soreness too is felt an inch or two to the right just above the navel. Such patients will trace with most anatomical accuracy, the course of the duodenum with their finger from the stomach to the loins on the right side and back again across the abdomen to the umbilicus.

In the second volume of his *Medical Histories*, pages 27, 8, 9. Dr. Ferriar has given us an account of his own case, which he believed to be a disease of the duodenum. Many of the symptoms coincide with those which I consider as indicative of a morbid state of this intestine. It is not a little curious as well as satisfactory to

to find that the plan he states to be useful was "to keep the body rather loose," which corresponds with what experience has taught me to be most advantageous, viz. to keep the intestines gradually and mildly excited as I have frequently inculcated. It is not uncommon to consider such a condition as indicating a primary disease of the liver, and if as is sometimes the case a swelling about the ankles should occur, it is regarded as a confirmation of the opinion which has been adopted. I believe this state to arise from a want of healthy action or from a total inactivity of the duodenum.

Under such circumstances the general practice of giving a dose or two of calomel with purgative salts, and subsequently stimulants and tonics produce no permanent good effects, for the duodenum relapses into its inactivity after a brisk purge; I have particularly remarked this of calomel, and all the

A A 2 symptoms

symptoms recur. The stimulus of the ammoniacal salts only relieves the present languor without enabling the duodenum to discharge its morbid and distressing contents; and should it so happen that spirits are unfortunately resorted to, they only hurry the morbid action into obstruction and inflammation, without ultimately removing the evils of accumulation and torpor. The preparations of steel create nausea and are sometimes vomited up, the simple bitter infusions lie cold and heavy on the stomach, and the addition of aromatic or spirituous stimulants do not remove the difficulty. Notwithstanding however that bitters disagree, yet when combined with a carthartic in such quantity as to give a gradual evacuating movement to the intestinal canal, they produce the happiest effects, ℥j. of quassia, and the same quantity or ℥ij. or ʒi. rarely the last, of senna infused in a pint of boiling water for
an

an hour, produce the composition I employ; fʒiss. of this infusion taken in the morning and repeated at noon, with pil. hydrarg. gr. iij. every night for about a fortnight, most generally remove this uneasy state of the duodenum.

In 1813, I was consulted by a clergyman of the county of Huntingdon who had laboured for a considerable time under all the above symptoms, except the swelling of the ankles. He had taken various tonics and purgatives and bitter infusions with no advantage. After premising an emetic it was, with some difficulty, I persuaded him to repeat the quassia he had formerly taken, as he not only had not found any relief, but was disgusted with any bitters from the uneasiness he felt after taking them. After having, however, taken the above formula for about ten days, he returned to me considerably relieved of his complaints, with the observation
that

that the quassia had agreed with my amendment. The infusion should not be prepared stronger than here directed, which is half the strength of that of the Pharmacopœia, for I have found that *strong* bitters are not salutary in their operation on the digestive organs, particularly that part of them I am now considering.

When this morbid condition of the duodenum is attended with some feverish heats and urine somewhat high coloured and rather scanty, instead of senna, I add some neutral salts to the bitter, and if I am not deceived in my observation, I can scarcely speak too highly of the sulphat of potash as the best. It appears to me to have a more specific effect upon the duodenum, than the sulphat of magnesia. I give ℥j. of it twice a day in the quassia infusion, and gr. iij. pil. hydrarg. with or without two grains of extract. aloes according to the state of the bowels. If much feverish irritation prevails arising,

sing, as I imagine, from some slight inflammatory action in the duodenum, the saline draught in a state of effervescence is substituted with the sulphat of potash, for the bitter infusion, with the happiest effects, and the pil. hydrarg. is given without the aloes. These may seem small doses, but the object is not to purge ; the ratio mendendi being merely to give evacuating movements, that the peristaltic action may be kept gradually and healthily excited, until the duodenum recovers its functions. In some cases there is a general torpor and coldness of the system with much pale urine and with the local symptoms already described. The evacuations are of the same dark colour with the addition of much mucus. Under such circumstances the neutral salts are not given, but to the quassia and senna infusion is added ʒss. of the sp. ammon. aromat. or fʒj. of some aromatic tincture, not omitting the mercurial aloetic pill at night. Under such

such a state too, very excellent effects are produced by taking in some inf. anthemid. every morning f3j. vini aloes with m. xv. liquor-potassæ. The pil. hydrarg. is then given without any combination. In some instances I give this pill only every other night, and sometimes, though rarely, not at all. By managements of these kinds, suited to the impression which the constitution has received from the varying state of the disease, in a few days tranquil sleep, a more cheerful mind, regular bowels, and healthy evacuations succeed to the mental agitation and irritability which had previously prevailed. A proper attention to diet and exercise is of course necessary. They will vary in their kind and degrees according to the different states of the disease as just described. I have known persons afflicted with this disease who were in the habit of taking frequent horse exercise. The clergyman above mentioned,

tioned, was accustomed to take horse exercise frequently.

It is here a matter of more than medical curiosity to form a discrimination between a liver disease on one hand, and a simple dyspeptic state of the stomach on the other, as contradistinguished from a morbid state of the duodenum simply. In some cases where the duodenal disease has existed for a considerable time the diagnosis becomes almost impossible, although an experienced practitioner may form a very good opinion ; because a morbid train of symptoms takes place, by which liver, duodenum, and stomach are blended in one affection, emphatically termed a disease of the digestive organs. In a simple disease of the stomach we have very little swelling or puffiness in the epigastric region, and when it does take place it is more to the left side. We have also eructations of wind and of acrid matters. These circumstances vary very

very considerably in a diseased duodenum ; a swelling is very apt to occur, the extricated gas finding a greater difficulty to escape either by regurgitation through the pyloric orifice, or downwards, from the particular situation of the intestine at the mesenteric ring. There are therefore no eructations of wind or of acrid matters in a dyspeptic state of the duodenum ; and when the puffiness is detected, it is a diffused swelling towards the right hypochondrium, being lost under the liver, and not extending to the left side, and circumscribed in that direction.

A diagnosis between affections of this intestine and the liver is more difficult, on account of the latter being so readily affected by a disease of the former, particularly when a slight jaundice has taken place, not from a diseased liver, but from the bile not finding a ready passage into the ill-conditioned duodenum ; an experienced physician must then form his
5 opinion

opinion from the accumulation of symptoms, and from his recollection of similar cases. If the symptoms of a liver disease, particularly the yellowness of the eyes, tension of the side, and lateritious sediment of the urine speedily disappear by the treatment, we may be perfectly satisfied that these hepatic symptoms are produced by duodenal irritation, and that the patient may be safely tranquillized on that ground by his anxious physician; if on the contrary, these symptoms are obstinate, and they seldom are without having so much disordered the general system as to awaken very early the suspicions of the physician, then that science and accumulation of facts, which taught him the discrimination, will immediately suggest to him the more active and appropriate remedies for the liver disease according to its kind and degree. With respect to the green urine mentioned by Dr. Ferriar, I have never witnessed it. I am not disposed

disposed to consider it as diagnostic, and probably it may be an accidental tinge of the urine, by the bile which produces the green jaundice *.

It has often happened that in a person, who labours under a chronic affection of the liver from some part of its substance being in a diseased state, and who will enjoy tolerably good health by care and management, this duodenal irregularity shall occur from causes which produce it in others independent of the condition of the liver. Experience has taught me to be very cautious here in analysing the true nature of the disease. The professional attendant, under the idea that the liver disease, which indeed does exist, is the cause of the distress, will give doses of calomel with active purges, but to his surprise finds, not only perhaps that no relief is obtained,

* See Dr. Baillie's paper on this subject. Page 143, Vol. V. of the Transactions of the College.

but that there is a jaundiced appearance of the skin on a subsequent visit ; arising from the excited liver having poured out more bile into the biliary ducts and duodenum, than has been carried off. I have frequently witnessed this in practice, and in the case of a lady whom I visited at Highbury in the winter of 1815, this was very remarkable. I was first called in to her some years previously, when under a salivation for a liver disease, which left, as is very often the case, an imperfect condition of that organ. She was attacked in the winter of 1815 with langour, fulness of the side, and with most of the symptoms I have already described. She had taken purgatives, and was told that she was again seized with her liver disease ; she was, however, soon relieved by the senna and quassia infusion. I may add, that in describing her symptoms, she traced with great accuracy the course of the duodenum.

Periodical mucous collections sometimes take place in the duodenum, with all the symptoms I have already described. I have a patient in Buckinghamshire, a farmer's daughter, between 20 and 30 years of age, who had been twice salivated for a supposed liver complaint. I was first consulted for her in 1812. She has, at different times, past much mucus by stool, after being distressed with uneasiness in the right hypochondrium, and with the other symptoms I have already detailed. She has been always relieved by the plan above-mentioned, when attacked with the duodenal disease; but as it sometimes attacks her with, and sometimes without, the feverish state, she varies her plan of management accordingly, and for which she and her apothecary have the necessary directions according to the above prescriptions. She very lately past, in one of these attacks, such a great quantity of mucus of a purulent appearance,

ance, as to induce her apothecary to think that an abscess had burst internally. She now enjoys much better health than she has done for many years ; and since delivering these lectures I have been informed that this patient is now in good health.

I have sometimes found it necessary with this duodenal irritation, to give a few minims of laudanum occasionally at night, to quiet the irritation, even after the other symptoms are subsiding, the morbid action appearing to continue from habit after the cause is removing or removed. Perhaps from a similar cause Dr. Butter found it useful to give the extract. conii in the infantile remittent fever, which I believe to be very much connected with this duodenal irritation. At other times, when the torpor does not appear likely to yield speedily to the plan, an ipecacuanha emetic has excited the chylopoietic organs, although

although nothing very particular shall be brought up by its operation.

Much has been said and written respecting the colour of the fœcal discharge being indicative of the colour of the hepatic secretion. A great deal of deception will, however, take place, particularly with persons who do not consider the effect which indigestion in, and a morbid affection of, the duodenum will have in altering the colour of the bile, however healthily it may be secreted. Different kinds of food, especially if not perfectly digested, will vary its colour and appearance very considerably; salads, greens, spinage, and French beans will give a morbid green appearance to the fæces, as if the hepatic secretion was much vitiated. On the contrary, persons who take a considerable quantity of milk, either from choice or on account of disease, will pass pale coloured whitish evacuations,

tions, as if the secreting actions of the liver were deficient in their functional powers, and I know that magnesia will give a uniform white colour to the fœcal discharge. These facts if not attended to especially in children, will certainly lead to erroneous and hurtful practice.

If under such circumstances there should be much derangement in the chylopoietic organs, particularly in the duodenum, a yellow tinge will be observed in the eyes with high coloured urine and uneasiness in the right hypochondrium, it will be believed that the patient suffers under a liver disease; active remedies will be had recourse to instead of the plan already described, which gives a gradually exciting movement to the intestinal canal, and relieves the duodenum.

I do not mention these facts to undervalue the information to be ac-

quired by inspecting the fœces, but to point out the necessity of inquiry into the kind of food taken, before a determination is formed upon this point. As a proof too that the colour is a fallacious sign and likely to lead to erroneous practice, I cannot avoid mentioning that different purgatives will produce a different colour in the evacuations, from exciting different glands to pour forth their contents. Whatever important information, as to medical practice, is to be derived from inspecting the fœces, it is of some consequence to remark, that the fœces of apparently healthy persons sometimes assume a dark slate colour a few hours after they are past; and again if the bowels have been costive for a day or two previous to the evacuation, the delay in the colon and rectum produces also what might be considered a degeneration in the appearance of the stool.

It

It is necessary in the first instance, particularly with children, to be somewhat minute in the examination; and in the second instance to examine the subsequent discharges in order to see the nature of the secretions recently made higher up the intestines, and which have not been delayed by constipation in the colon and rectum. To those who are desirous of considering the study of the profession as a medical science, and not as a conjectural art, and are anxious to detect the precise nature of diseases, the symptoms of which, particularly in the abdominal viscera, are often involved in much obscurity, these observations will not appear trifling or unimportant.

Having thus made some observations on the diseased condition to which the duodenum is more particularly liable, it may be of use briefly to notice how it is affected by the morbid condition of the adjoining organs. A

greatly distended stomach from flatus or other causes would press the colon against the part where it enters the mesenteric ring, and impede the discharge of its contents into the jejunum; and this effect would be more completely produced if the colon contained hardened fœces: in like manner the weight and bulk of enlarged mesenteric glands which lie so numerous about its termination, would also cause inconvenience by pressure. A greatly enlarged liver, or distended gall-bladder would cause distress in it, and I have already noticed the manner in which its important angulated bend would be confined between the right kidney and distended colon, and as the ductus communis enters here, would be the cause of jaundice. Under all or any of which circumstances a perplexity in the diagnosis would be added.

In whatever point of view therefore, the duodenum be considered, whether

as to its situation, connections, tortuous course or functions, it is an organ of very great importance, and one which particularly claims attention, and that therefore a consideration of its functions, uses and diseases cannot fail to be of much utility.

Mode of Examining the Tract of the Duodenum, with an Explanation of the Plates.

TRACE the intestines backwards, beginning at the ileum where it enters the colon, gradually and slowly unfolding their convolutions in a retrograde direction, till you get to the part where the duodenum emerges from the ring in the mesentery. It is here where the jejunum begins, around this part throw a ligature, tightly secured, then make a small opening into the stomach about two or three inches from the pylorus, and introduce the nozel of a small pair of bellows about one inch from the pylorus, passing a ligature round the nozel and that part of the stomach in which it is included. This is a better and a more convenient mode than making use of the blow-pipe, as it keeps the intestine more uniformly and permanently distended for the purpose of taking a drawing, and for more accurately delineating the state of parts, because the air from the lungs soon losing its caloric to the cold body, and the carbonic acid gas with which it is charged, being

being absorbed by the moisture of the parts, the intestine becomes flaccid by the loss of the rarefaction, and the absorption of the carbonic portion, of the air blown into it; the *relative bearing* of parts is not then so clearly seen.

The duodenum is thus inflated cautiously and slowly, so as to remove it as little as possible from its natural situation. After having so done, a ligature is then past round that portion of the stomach which lies between the pyloric orifice and the nozel of the bellows which is now withdrawn. The colon is then divided in the centre between two ligatures to confine the dirt which would otherwise escape. After being thus divided and separated, one portion is thrown to the left and the other to the right side of the body, as is represented in Plate I. The omentum of course being removed; now upon raising the great curvature of the stomach and the liver, you will have a very good view of those parts of the duodenum which are not covered by the envelopments of the mesocolon and mesentery. By cautiously dissecting the mesentery, and by dissecting out the mesocolon where it envelops this intestine, you are much rewarded for your pains in having an opportunity of observing its really beautiful and tortuous course from its commencement at the pylorus, to its termination just below the centre point of the arch of the colon. As the pylorus is situated about the height and to the right of the tenth dorsal

dorsal vertebra, and as the duodenum terminates to the left of the twelfth dorsal vertebra, after having taken a curvilinear direction to the right, it almost describes a complete circle in its course.

PLATE I.

This Plate represents the duodenum in its proper situation, the parts which exclude it from the sight being removed—from Claussen. I am indebted to Mr. Charles Bell for permission to copy this Plate in Sandifort's Thesaurus.

a a a a a a. The colon bisected and thrown to the right and left side with the mesocolon attached to the left portion.

B. The stomach with its great curvature raised.

C. C. The Liver.

D. The Gall-bladder.

E. E. The Duodenum descending by the side of the gall-bladder, the indented part just above the first E. being the pylorus.

F. The angulated part of the duodenum where it is attached to the capsule of the right kidney, and where the biliary and pancreatic ducts empty themselves.

G. That portion of the duodenum which ascends from the kidney, passes across the abdomen and under the mesentery.

H. The end of the duodenum coming from under the mesentery at its ring.

I. Aorta.

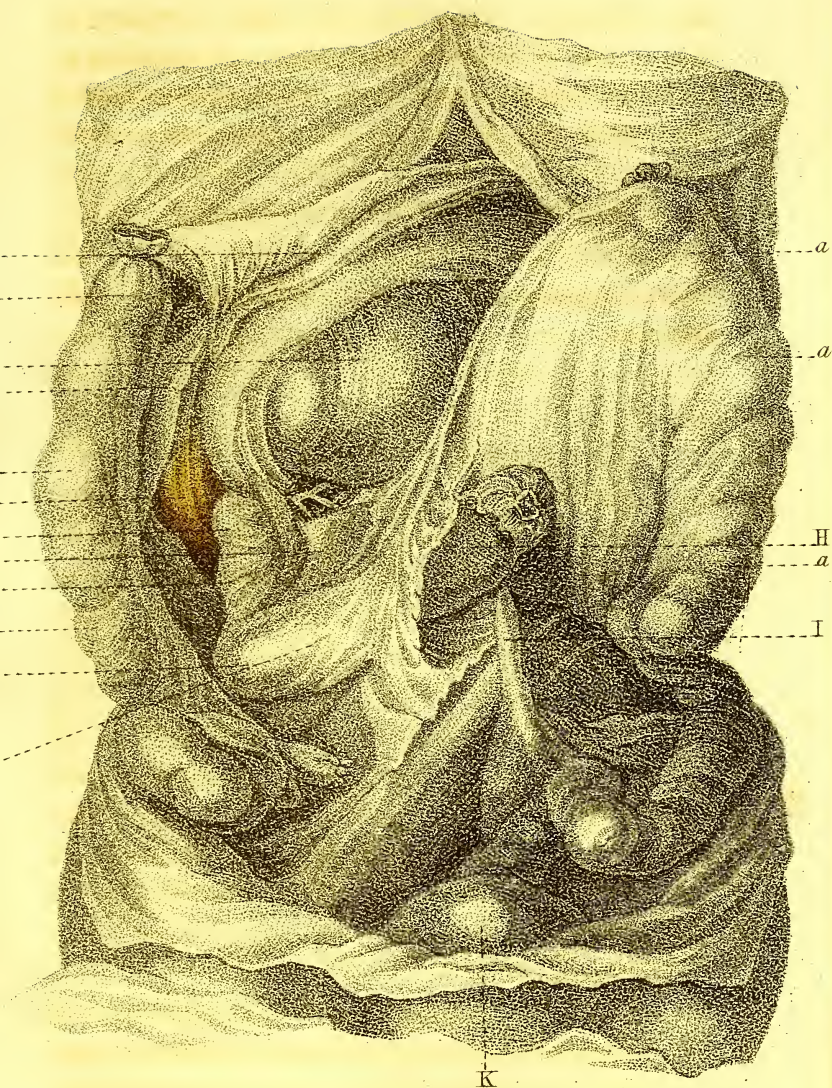
I. *Aorta.*K. *Bladder.*L. *Pancreas.*

PLATE II.

This Plate represents the duodenum very much distended with air blown into it through the cardiac orifice of the stomach, which is itself thereby inflated. The preparation was made after the manner described above, by the kind permission of Mr. Carpue at his dissecting room, and the drawing was taken from the body by Mr. Paris, an ingenious anatomical draughtsman, 29, Hyde Street, Bloomsbury. It will be seen by the Plate how a greatly distended duodenum will rise and press against the liver and gall-bladder, as well as against the colon which passes directly across and above it; and on the contrary, how a distended colon as it rises from its head at the right hip, will press the angle of the duodenum (F. of Plate I.) against the right kidney. The colon is here, Plate II., very small, because it was punctured with the intention of letting out all the air, that the inflated duodenum might be brought more distinctly into view. The duodenum is seen rising from its concealment from under the colon where the jejunum begins, represented where the ligature is tied.

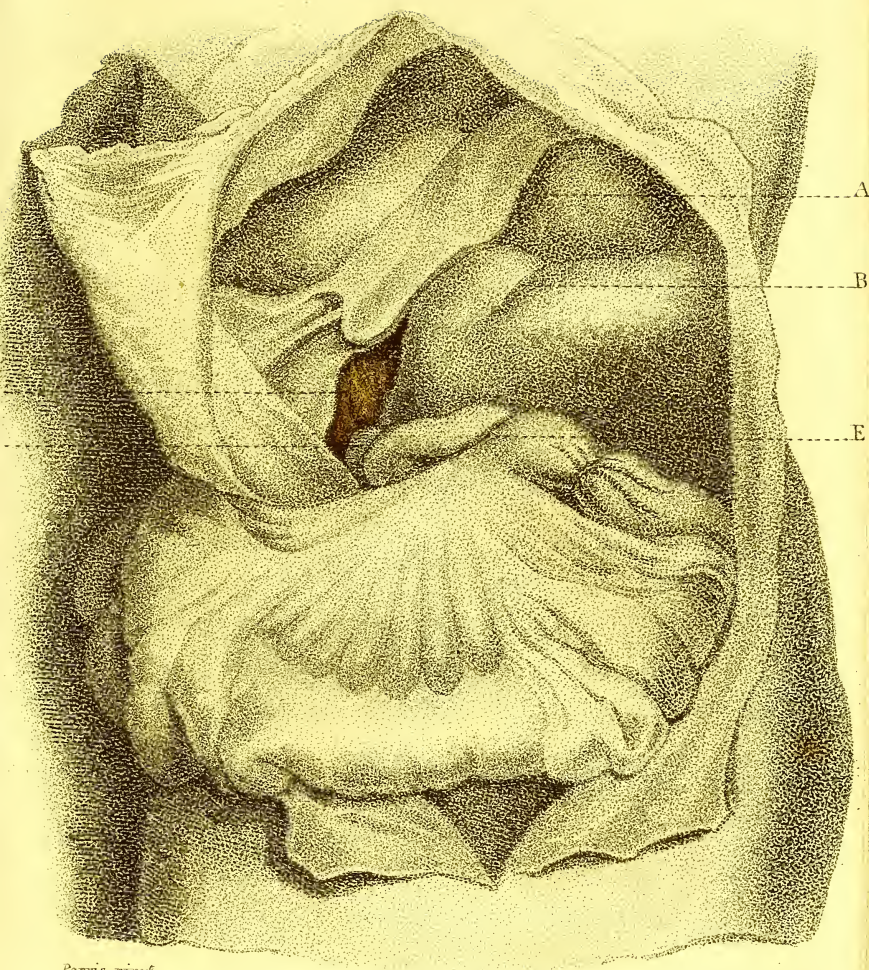
* * * Both these Plates are taken from adult subjects. In young children, the liver projects a great deal

deal more over the duodenum than in adults, as that organ in them bears a larger proportion to the other viscera: and from what observations I have been able to make, the duodenum in them is more horizontal in its position before it makes its descent to the kidney; consequently any distension or irritation of the duodenum will in them have a greater effect upon the liver and gall-bladder.



From Claupfen

L.T. Wedgwood sculp.



Parriss pinx.

J.T. Wedgwood sculp.

A. The portion of the body, over which the other end of this dotted line passes, is the posterior part of the stomach greatly inflated & pressed upwards. The Duodenum is seen rising above the pyloric indentation. The great curvature of the stomach & the lower part of the Pylorus being covered by the Mesocolon.

B. The Pylorus, C. Gall bladder.
D. at the other end of this dotted line is a fold of the Colon, behind & above which the Duodenum is seen descending, passing under & pressing against the Gall bladder.
E. at the other end of this dotted line the Duodenum is seen passing from under the Colon thro' the Mesenteric ring.

XIV. *Observations on the Hour-Glass Contraction of the Uterus.* By JOHN C. DOUGLAS, M.D. *Licentiate of the College of Physicians, Ireland. Presented by the Registrar.*

Read at the COLLEGE, March 27, 1820.

“I CANNOT help regarding the neck of the uterus as a distinct and independent part, from the body and fundus; and as having its own peculiar laws, and actions; and that this separation of powers is absolutely necessary to the explanation of some of the phenomena, exhibited by health and disease, and the influence of certain agents on these parts.”

“W. P. DEWEES.”

It will readily be recollected by those gentlemen, who were pupils in the
Lying-

Lying-in-Hospital of this city (Dublin,) during the years 1810, and 1811, and by many others, whom I have, on various occasions, addressed on the subject of midwifery, that I have long entertained an opinion similar to the above doctrine, and hereby endeavoured to explain some of the more frequent causes of protracted labours. But, I more particularly insisted upon an independency of action, when advocating my views of that condition of the uterus, termed hour-glass contraction. A more satisfactory explanation of which is the object of this paper.

On reflection every one must feel disposed to admit, that we early imbibe habits of receiving the opinions of our teachers and seniors, without venturing to question their propriety or to examine their solidity. This error has perhaps been the chief of many causes, which have retarded the progress of improvement in medical,

cal, as well as in other sciences; and has led to the retaining of many absurd opinions, which ought to have been long since exploded.

Whether the present exposition will have any claim to the rank of a doctrine destined to supersede an erroneous theory, by explaining more satisfactorily the nature of this untoward occurrence in midwifery, must rest with the judgment of the experienced practitioner.

We all know that the placenta is usually expelled within a short period after the birth of the child, without any particular interference on the part of the accoucheur. Cases of retention* are not now very numerous; its occurrence gradually becoming less frequent, in consequence of increasing improvement in this branch of the medical profession.

* By the term retention or detention is to be understood, untoward or protracted delay of the placenta, after the birth of the child.

It

It has been a maxim universal with authors and teachers of midwifery, that detentions of placenta are attributable to three causes, and are occasioned indifferently by any one of them, viz. irregular, or hour-glass contraction of the uterus, morbid adhesion of the placenta, and inaction of the uterus. I do not recollect any computation of the relative numbers occasioned by these respectively ; but I believe it to be the received opinion, that a greater number result from this supposed hour-glass contraction, than from either of the other two causes, or perhaps than from both combined.

In questioning the accuracy of this opinion, I do not wish it to be thought that I have not, in the course of practice, met with the hour-glass contraction of the uterus : I freely confess, I often experienced its existence, and in consequence thereof, I have often felt difficulty, in reaching the placenta.

But,

But, notwithstanding this concession, I presume to adduce as my opinion, that a placenta has rarely, if ever, been primarily retained, by this cause. I might therefore, in a pathological sense, reduce the causes of detention from three to two: to morbid adhesion of the placenta, and to inaction of the uterus.

If this position be correct, it naturally follows that the occurrence of this hour-glass contraction, should only be considered as a secondary cause of detention; its formation being merely the result of the undecided manner, in which the practitioner introduces, or attempts to introduce, his hand, with the intent to extract a placenta that had been retained by one of the other two causes, which I might denominate primary causes.

It is not, however, my intention here to contend for any new arrangement of assignable causes of detention. In
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the first place, because I cannot positively assert that the hour-glass contraction never was a primary cause, although such be my opinion. And, secondly, because, as such contractions will hereafter occasionally occur, although less frequently than heretofore, from heedless or injudicious management; it may be desirable that the precepts inculcated for such events, should remain extant.

And yet, I may remark, notwithstanding there are particular rules of conduct prescribed for the accoucheur on such occasions, he must in practice ever act from the incidental circumstances of individual causes, and not be entirely guided by insulated rules, or nosological distinctions.

With regard to the validity of my position, I conceive it to be but of trifling moment; whether, in any case of hour-glass form, we admit that the muscular fibres of the uterus had been
irritated

irritated into this contraction, by the protracted presence of the practitioner's hand within the cervix uteri ; or, whether we suppose the stricture had been occasioned, by his injudiciously irritating the vagina, in minor attempts to bring down a placenta, that had been detained by some unavoidable cause. The inference in either case, would be the same, that this hour-glass contraction had not been the original cause of retention.

Anatomists and teachers of midwifery divide the uterus by imaginary circular lines into three parts, viz. into the fundus, the body, and the cervix ; the upper section is termed its fundus, the lower its cervix, and the intermediate (the limits of which are not positively defined), its body.

As we are left at liberty to dispose of those lines on the uterus, as we please, I would so arrange them, as to apportion $\frac{2}{7}$ to the fundus, $\frac{3}{7}$ to its body, and $\frac{2}{7}$ to the cervix. By this division,

the upper and lower sections on the long diameter of the uterus, are of equal length; and the middle section is equal to one and half of either.

The foregoing division would apply to the uterus itself in every condition, but as the uterus and vagina, at the period of delivery, form one continuous and uninterrupted canal; I would then divide the combined two, into four portions; and of the entire, assign $\frac{2}{10}$ to the fundus, $\frac{3}{10}$ to the body, $\frac{2}{10}$ to the cervix uteri, and $\frac{3}{10}$ to the vagina. In this arrangement, the two upper sections, the fundus and body of the uterus, are equal to the two lower, the cervix uteri and vagina.

This combined anatomical view of the uterus and vagina at the period of delivery, instead of considering of each apart, appears to me requisite for a correct conception of many of the phenomena of labour.

And such arrangement appears naturally to exist in the constitution of
the

the parts themselves. For we find the fundus and body thus to comprise the entire of the thickly muscular substance of the uterus, whilst the portion allotted to the cervix partakes more nearly of the structure of the vagina, than of the upper portions of the uterus.

Thence the propriety of anatomically “ regarding the neck of the uterus as a distinct and independent part from the body and fundus ;” and I myself think that instead of considering, at the period of labour, the entire of the uterus as one organ, and the vagina as another ; it would be more judicious and natural to consider the fundus and body of the uterus as one, because possessing a common structure, and the cervix uteri and vagina as another, being also nearly of a common structure.

And I trust the eligibility of this arrangement will not be less perspicuous, in a physiological point of view ;

as the functions of the cervix uteri, during labour, are the very reverse of those of the fundus and body. The fundus and body exert a contractile and expulsive action; whilst on the contrary, the cervix relaxes in sympathy with the vagina. The cervix relaxes and must relax, otherwise the foetus could not get exit; and the fundus and body must contract, otherwise the foetus would not be expelled.

Although any discussion on the general subject of labour would be a deviation from the avowed object of this paper; yet as connected with the physiology of the uterus, I cannot omit to remark, that I have long considered the protraction of tedious labours to arise more frequently from want of yielding elasticity in the cervix uteri and vagina, than from deficiency of expulsive action in the body and fundus.

To my admission already expressed, that I have met with the
hour-

hour-glass form of the uterus, in the course of practice, I will here add, that I have even felt the lower chamber (as it is termed) so nearly arched by the contraction, as to convey the impression of my hand having there reached the fundus itself. But, notwithstanding this fact, I object to the generally received opinion of such stricture being produced, by any spontaneous action of the uterus. I would now beg simply to ask the candid practitioner, did he, in any case, after having overcome this stricture, find the placenta in the upper chamber detached?

If he answer no, an answer which I anticipate, that answer would be almost tantamount to a confirmation of my doctrine. The utmost that the most obstinate could then contend for, would be that this hour-glass contraction is occasionally a concurrent cause of detention. But, if independently of the contraction, there
invariably

invariably exist a cause, and in itself sufficient to account for the retention; such fact must indubitably smooth the way, for the reception of my explanation.

Practitioners have observed, in the operation of turning the fœtus in cross-birth, it frequently is not necessary to introduce the fore-arm further within the os externum, than midway or so from the wrist to the elbow. Hence it is inferred, if only so much of it be required to be introduced in an operation, when the uterus is distended with a fœtus, not more if so much will be required merely to bring away a placenta, when the principal cause of distention no longer remains.

But, although the principal cause of distention be removed by the birth of the child; yet, the uterus at this time is not in length decreased. Moreover, in cross-birth we can often effect delivery without our hand
going

going nearly so high as the fundus. Whereas, in the operation in question, it is always requisite that the convex hand should press against the farthest boundary of its cavity. Our object being not only to extract the placenta, but also to excite the uterus into regular and wholesome contraction.

The practitioner however, instead of effecting this object introduces (I mean occasionally) his hand only so far as to be within the cervix uteri; and, whilst searching there in vain for the placenta, he unconsciously irritates the lower edge of the thickly muscular part of the uterus, into action. The hour-glass contraction is the usual result of this irritation; and, by the time the practitioner has discovered his error, a barrier is thus opposed to the further progress of his hand.

In some time after I had adopted in theory the opinion I am here advocating,

advocating, I availed myself of an opportunity in a case of retention, to introduce my hand only so far as to be within the cervix uteri; and, having delayed it there designedly, as I had on former occasions done inadvertently, I found the uterine cavity gradually assuming the hour-glass form. I then quickly passed up my hand to the fundus, without allowing the constriction to close so much as to materially impede its progress.

Having advanced so much in favour of my opinion of the cause, I shall now say a few words on the locality, of this stricture. Which locality I consider not to be at the central circumference of the uterus, as is more generally supposed; but at the centre of the cone formed conjointly of the uterus and vagina.

It had always seemed to me an enigma, that this apparent inclination to oppose the hand of the accoucheur,

coucheur, thereby subjecting the suffering female to much additional pain, should be inherent in a circular band of fibres at the centre of the uterus; and that the same structure of substance, both above and below this belt, should remain quiescent and relaxed.

But this stricture does not form from the middle circumference of the uterus; it is formed by the lowest verge of its thickly muscular substance at the line of demarcation of its body and cervix. Which line in my arrangement of parts, is at an equal distance from the os externum of the vagina and the farthest part of the fundus uteri.

Thus then it would appear, that the upper chamber comprises in its formation the entire of the body and fundus; whilst the lower chamber engages only the cervix uteri and vagina; and yet, these two compartments

ments are, as nearly as may be, equal in capacity.

I ascertained the foregoing, in the following manner. Having in a case of this kind, overcome the stricture and gotten my hand into the upper chamber, it soon contracted and threw off the placenta; and shortly after its capacity was so much diminished as merely to contain my fingers. I was then enabled by pressing my thumb against the thinner structure, to take (as it were) hold of the edge of the thickly muscular substance evidently terminating there; and, I could easily imagine from the point of my arm then at the os externum, that my finger and thumb rested on that part where the stricture had been. And likewise, on withdrawing my fingers from the contracted upper chamber, and rolling my hand within the lower, I found it equally capacious, as it had been previously to the diminution

tion of the upper. The stimulus of my hand not similarly exciting it to contract.

Can any stronger facts be required to prove that the entire of the thickly muscular part of the uterus had been engaged in the formation of the upper chamber. For, if the lower or any part of it had been of similar structure with the upper, surely it would have contracted upon application of the same stimulus, the hand.

I will now briefly state it to be my opinion :—

That the remote cause of the uterus assuming the hour-glass form, is a miscalculation of the distance (which is not less than fifteen inches) at this period, from the os externum to the fundus of the uterus.

That the exciting cause is irritation, produced either in the vagina, by injudicious pulling at the umbilical cord ; or, in the cervix uteri, by the

the accoucheur's hand searching there in vain for the placenta.

That the proximate cause is a spasmodic constriction of the muscular fibres of the uterus at the lower verge (not at the centre) of that section termed its body, and just where it ceases to be thickly muscular.

Thence, I conclude that this hour-glass contraction is not produced by any principle of action inherent in the uterus itself; and that whenever it does occur, it is caused by mismanagement.

Therefore, in order to avoid such occurrences, the practitioner should always refrain from exciting unnecessary irritation.

And, in those few cases of unavoidable retention of the placenta, wherein it may be necessary for the accoucheur materially to interfere, he should, having first cautiously inserted it within the vagina, push
his

his hand briskly up to the very fundus of the uterus. And, in this operation, he should direct the hand forward towards the umbilicus; ever bearing in recollection that the axis of the uterus, as well as the axis of the pelvis, inclines at a considerable angle to the axis of the trunk of the body.

I have omitted to speak of the management of the placenta, generally, as not immediately falling within the limits of this paper, and as being unnecessary, in consequence of the ample directions detailed on that subject, in Denman's System of Midwifery. Which publication, together with his aphorisms, I presume to be in the possession of *every* accoucheur.

XV. *On the Necessity of Caution in the estimation of symptoms in the last stages of some diseases.* By SIR HENRY HALFORD, BART. M.D. F.R.S. *Fellow of the Royal College of Physicians, &c. &c.*

Read at the COLLEGE, March 27, 1820.

IT is of great importance to the character of a physician to be able to foretell the issue of a disease, and it is of essential comfort to the friends of his patient, if the malady has been an incurable one, that he should have apprised them that he expected a fatal termination of it. Where this has not been done, the poignancy of the grief of the family is liable to be increased by a reflection that the physician himself was taken by surprise; and, therefore, probably had not made use of all the resources of his art, by which the catastrophe might have

have been prevented. On the other hand, if with discretion and feeling he had disclosed his apprehensions of the fatal result, their sorrow would be mitigated by a conclusion that every thing had been done to save life which skill could suggest, and their future confidence in that physician's assistance would be confirmed and increased.

The art of physic has been called a conjectural one; and so it is, if that term be construed to mean only that uncertainty which attaches to all reasoning from what has happened to what will *therefore* happen again, to inferences drawn from general results to particular instances; but this is the only legitimate reasoning of which the science of medicine, in common with many other sciences, admits; and it suggests therefore, the necessity of recording facts, carefully ascertained by repeated experience. Were this done by every physician of
 extensive

extensive practice, what appears extraordinary in a single instance would become familiar by repeated observation, and the difficulty of prognosticating would be materially diminished; to the great credit of physic, and to the satisfaction of its professors.

It often happens at the latter end of some diseases, both of an acute and a chronic nature, that appearances present themselves of a very equivocal and delusive nature; with which the issue of the malady does not correspond. This is most frequently the case when the resistance of the constitution against the influence of the disease has been long protracted, or when the struggle, though short, has been very violent. Here, a pause in nature, as it were, seems to take place; the disease "has done its worst," all strong action has ceased, the frame is fatigued by its efforts to sustain itself, and a general tranquillity

tranquillity pervades the whole system. This condition of comparative ease, the eager wishes of friends misconstrue into the commencement of recovery, and the more readily so, as the patient himself being appealed to to confirm their anxious hopes, having lost some of his sufferings, admits, perhaps, that he is better.

The physician, however, must not be so misled. He must exercise his soundest judgment under such circumstances. He must satisfy himself that there exists real ground of improvement. For if he lead himself to such hopes unwarily, he compromises his own character, and runs a risk of aggravating exceedingly the painful feelings of the family.

The junior part of our profession, those who have not yet lived as many years in the exercise of it as I have done, will take it in good part, I hope, if I point out some maladies in which such delusive appearances

are most apt to take place, and suggest that caution to them in the estimation of symptoms which I have found it necessary to employ myself.

I have seen this fallacious truce in four or five instances of inflammation of the brain, particularly where the membranes which cover it have been inflamed, producing phrenzy.

A young gentleman of family, about twenty-five years of age, took cold whilst under the influence of mercury. The fever increased daily until it was accompanied at last by so much excitement and delirium, as made it necessary to use not only the most powerful medicines, but also personal restraint. At length, after three days of incessant exertion, during which he never slept for an instant, he ceased to rave, and was calm and collected. His perception of external objects became correct, and they no longer distressed him, and he asked, pressingly, if it were possible

possible that he could live? On being answered tenderly, but not in a way calculated to deceive, that it was probable he might not, he * dictated most affectionate communications to his parents abroad, recollected some claims upon his purse, "set his house in order," and died the following night. The reason why so unfavourable an opinion was entertained of his state, was, that the apparent amendment was not preceded by sleep, and was not accompanied by a slower pulse, two indispensable conditions on

* My friend Dr. Heberden, when I mentioned this case to him, shewed me a note which his father had received from a patient, written in the interval of the subsidence of a paroxysm of phrenzy and his death, which happened about fifteen hours afterwards. The note is of some length, and is written correctly.

See the chapter of Aretæus on the *Kavros*, as remarkable for the sublimity of the ideas which it contains, as for the beauty of the Ionic Greek in which they are expressed.

which only a notion of real improvement could be justified. But here was merely a cessation of excitement occasioned by a diminution of power, and by a mitigated influence of the action of the heart upon the brain.

In inflammation of the bowels generally it is so notorious, that mortification often follows a cessation of pain, that I do not think it necessary to dwell upon this form of disease with a view of cautioning physicians ; but in that partial inflammation of the intestines which a strangulation of a portion of it in hernia produces, how often have I had occasion to deplore the disappointment and broken hopes of relatives, who, having been made happy by the assurance of the surgeon that he had reduced the protruded bowel, and that now all would be well, in only a few hours afterwards were doomed to lament the patient's death! It is an invariable rule with me still to consider life as
in

in jeopardy, until the intestines shall have performed their functions again, all irritation having left the stomach, and the skin remaining universally and equally warm.

An abscess in the liver, connected with gall-stones in the gall-bladder, will sometimes assume the type and character of a regular intermittent; both in the periodical recurrence of the paroxysm, and in the succession of its stages. I have seen it treated as an intermittent, with the remedies usually administered to prevent the return of the fit. But a careful attention to the history of the previous symptoms will enable the physician to discover the essential difference. He will learn that there was a well marked attack of inflammation in the region of the liver in the first instance, which has ended in the formation of an imposthume. Besides, the affection of the brain, in the second stage, that of re-action, will be observed to exceed

exceed in severity that which attends any sure intermittent, amounting, as it does, to an apoplectic stupefaction, under which, in fact, the patient dies ; and lastly, he will be struck by the extraordinary alteration of the colour of the skin, which from being fair becomes of a deep brown tinge in the paroxysm. I saw three instances of this disease of the liver in the year 1805, all of which assumed the appearance of intermittent fevers. The subjects of them were females, at that period of life when the catamenia had just ceased. Two of the patients died in the fourth attack, and were examined after death. The life of the third was protracted a fortnight by the matter of the abscess having made its way into the channel of the intestines, and being passed off in a large quantity daily by stool.

I will now mention a chronic disease, dropsy in the chest, in attending which a physician should be on his guard

guard when he gives an opinion in the advanced stages of it. We have all seen, in cases of hydrothorax, a most material mitigation of the embarrassment in breathing ensue on the legs swelling, so great a one indeed, that the patient and his friends have flattered themselves that no ill remained beyond the mere hydropic enlargement of the lower extremities. I have to remark, that if this swelling of the legs disappear without an increased discharge of urine, the patient generally dies very soon, and most frequently suddenly. Whereas if an ample increased secretion by the kidneys *follow* the relief of the dyspnœa, then every good hope of a temporary recovery, at least, may fairly be entertained; though it should be acknowledged that this species of dropsy, above all the others, is most apt to return.

Another disease, which happily we see now very rarely, the confluent small

small pox, requires a very guarded prognosis at a certain stage of it. The physician may fairly acquiesce in the fears of a family, when, on the completion of the eruption, he sees the face and breast one mass of disease, and may most reasonably doubt the capability of the constitution to mature and perfect so large an eruption. But he must not hold out unfounded hopes to the parents if the malady proceed in the next stage, in a most satisfactory manner, beyond his expectations. The pustules ripening fully, and the process being complete—for alas! at this very moment it may be, the patient is sinking—is dead! the powers of his constitution being exhausted by the efforts it has made, and no longer equal to the accomplishment of a protracted cure.

Analogous somewhat to the maturation of small pox is the reparation of the skin when it has been destroyed extensively by burning. I have seen
a good

a good many instances of this misfortune, four of which proved fatal, and yet in every one of the four the wound had healed, with the exception of the space only of a crown-piece. Three of them were aged women, who were burnt principally on the trunk of the body; the fourth was a girl of seventeen years of age, who was burnt from the heel to nearly the top of the inside of the thigh. The girl became hectic in the last fortnight of her life. The others died, "no warning given" by any mark or particular symptom of danger. It is prudent therefore to consider a patient as still in hazard under such circumstances, until the wound has been entirely healed for some time, and the constitution has recovered its usual energy.

I will trespass on the patience of the College a moment further whilst I mention one more disease, which, though it does not fall precisely within the class of those which are apt to manifest

manifest fallacious appearances in their last stages, yet is at once so dangerous and so soon fatal, that every physician should be aware of it, the paralysis of the kidney. It is not of frequent occurrence, I presume, as I have seen only five instances of it in twenty-seven years. The last was about two years since; and as it was an exact copy of all the others which had fallen under my notice, I will detail it shortly.

A very corpulent robust farmer, of about fifty-five years of age, was seized with a rigor which induced him to send for his apothecary. He had not made water, it appeared, for twenty-four hours; but there was no pain, no sense of weight in the loins, no distension in any part of the abdomen, and therefore no alarm was taken till the following morning, when it was thought proper to ascertain whether there was any water in the bladder, by the introduction of the catheter;
and

and none was found. I was then called, and another inquiry was made, some few hours afterwards, by one of the most experienced surgeons in London, whether the bladder contained any urine or not, when it appeared clearly that there was none. The patient sat up in bed and conversed as usual, complaining of some nausea, but of nothing material in his own view; and I remember that his friends expressed their surprise that so much importance should be attached to so little apparent illness. The patient's pulse was somewhat slower than usual, and sometimes he was heavy and oppressed.

I ventured to state that if we should not succeed in making the kidneys act, the patient would soon become comatose, and would probably die the following night; for this was the course of the malady in every other instance which I had seen. It happened

pened so; he died in thirty hours after this, in a state of stupefaction.

All the patients who have fallen under my care in this disease were fat corpulent men, between fifty and sixty years of age; and in three of them there was observed a remarkably strong urinous smell in the perspiration twenty-four hours before death. Only one of them had complained of previous nephritic ailment. He had suffered frequently, and had passed several small calculi, but there was no difference in the progress of his symptoms when the paralysis had once taken place.

If any water, however small the quantity, had been made in these cases, I should have thought it possible that the patients might have recovered; for it has often surprised me to observe how small has been the measure of that excrementitious fluid which the frame has sometimes thrown
5 off,

off, and yet preserved itself harmless; but the cessation of the excretion altogether, is universally a fatal symptom in my experience, being followed by oppression on the brain.

The observation of other physicians will supply them, no doubt, with abundant proofs of the necessity of that caution which I have suggested above. My own memoranda indeed would furnish many more instances; but I must confine myself to the limits of a short paper, and leave room for the more valuable communications of my colleagues.

XVI. *Case of death by poison, wherein Impregnation had taken place, and the Ovum was detained in the Ovary; with an Engraving descriptive of the appearances on dissection.* By EDWARD STANLEY, Assistant Surgeon and Demonstrator of Anatomy at St. Bartholomew's Hospital. Presented by the President.

Read at the COLLEGE, June 2, 1820.

THE case of a foetus, of the age of four months, which had grown in the ovary, having been lately read before the Royal Society, the following instance of an ovum, at a much earlier period discovered in the ovary, may not be unacceptable to physiologists, inasmuch as it completes the history of the ovarian extra-uterine foetus, and gives all the proof that can be required from this source in confirmation of the present theory of conception.

A woman, about thirty years of age, took laudanum to an amount that
could

could not be clearly ascertained, nor could it be known what period had elapsed before professional aid was afforded to her. She was found insensible, her countenance was livid, muscular power was completely lost, the pulse was quick and feeble, and the pupil contracted. A catheter was passed into the æsophagus from the nose, and through this instrument, half a drachm of sulphate of zinc dissolved in water, was injected into the stomach without the desired effect; the repetition of the dose was alike inefficient. One drachm of compound spirit of ammonia, with some compound tincture of cardamom was next injected through the catheter; on this being repeated, a small quantity of fluid was discharged from the stomach by the mouth, not however decidedly smelling of laudanum. She was now placed in the warm bath, but with no good effect; other means were used to rouse the system, but to

no

no purpose ; there was not at any moment the slightest appearance of returning animation ; the pulse became gradually more feeble, and death took place in five hours from the time that the means of relief were commenced. It may here be mentioned that upon inquiry, no other motive for self-destruction could be assigned, except the occurrence of impregnation from illicit intercourse, which was considered very likely to have taken place.

On examination of the body, water was found in the cellular tissue of the pia mater covering the greater part of the cerebrum ; in other respects, the brain had no unusual appearance. The stomach was contracted and filled by fluid, not having either the colour or smell of laudanum, but rather appearing to be formed by the mixture of the different articles which had been introduced into the organ. When this fluid was washed away, the
mucous

mucous membrane throughout presented a red colour. Upon the edges of the rugæ, the redness was deepest, and evidently from the staining of the membrane by the tincture of cardamom; the general redness of the membrane was produced by effusions of blood into the cellular tissue, connecting the muscular with the mucous coats of the organ. The blood-vessels ramifying in the broad ligaments of the uterus were remarkably distended, and in much greater number than are seen in the natural state of the parts. The uterine organs being removed and carefully examined, the following circumstances were noticed in them.

The uterus in its general dimensions was larger than it usually is in the unimpregnated state; the convexities on its anterior and posterior parts were increased, which gave to the body of the organ somewhat of a globular form; its substance was soft

and spongy except in its neck, where it was more firm. The internal surface within the body of the organ was covered by a layer of yellowish white substance, of a soft and pulpy texture, in fact having the usual characters of the decidua; the thickness of this layer was nearly half an inch. Within the neck of the organ, the internal surface was covered by numerous small portions of jelly, some of which were seen exuding from the orifices of the lacunæ; the quantity of this jelly was not sufficient to close the passage into the uterus. The Fallopian tubes were enlarged and remarkably twisted. Within the cavity of each was seen a number of loose floating processes, formed by folds of the inner membrane, and extending along the tube from its fimbriated extremity to within an inch of its termination in the uterus. These folds had a beautiful appearance, which may be very well compared to the soft villous surface of
the

the *valvulæ conniventes* in the jejunum; and as their direction was for the most part length-wise in respect to the tube, we may infer them to have been formed by the enlargement of the longitudinal ridges in the inner membrane, which have been considered by some physiologists to be of a muscular nature. The separation of the folds discovered deep clefts between them, which seemed very well calculated to detain the ovum in its passage to the uterus.

The opinion that the ovum is ordinarily detained for some time in the Fallopian tube, receives support from the observations made by Mr. Cruikshank on rabbits, who found that a considerable period intervened between the escape of the ova from the ovaries and their entrance into the uterus, during which they were engaged in the Fallopian tubes. The left ovary was considerably the largest, and at its posterior part was a rounded prominence distinct from the general

fulness. The coverings of the ovary in this situation were loaded with tortuous vessels, and from careful examination, it was clear that there had not been any aperture in the external membrane. Its surface was perfectly smooth. The membrane being divided over the middle of the prominence, a distinct cyst was exposed, and within this cyst was discovered an ovum. The internal surface of the cyst was smooth and polished, its external surface was firmly adherent to the substance of the ovary. The ovum was simply in contact with the cyst in two-thirds of its circumference; in the remaining third, it was united to it so closely as to be inseparable. The chorion and amnios were perfectly distinct, and by the aid of a magnifying glass, vessels filled with blood were seen ramifying upon the former. The cavity of the amnios was filled by a yellowish honey-like matter, but the fœtus could not be discovered.

discovered. Lastly, it is to be noticed that around the ovum, the ovary was for some distance loaded with blood, apparently effused into its substance. The section of the right ovary exposed numerous vesicles, such as are usually termed ovula Graafiana, and one large cyst adequate to contain a horse bean, filled by watery fluid, and having fleshy excrescences growing from its internal surface. The appearance of this large cyst induced the opinion that it was the effect of disease.

It is interesting to enquire, upon what evidence we are to depend as conclusive of the fact that impregnation has taken place, when the examination of the uterine organs is made at a period too early for the ovum to be distinctly seen. The existence of the corpus luteum in the ovary was formerly regarded as a sure test of impregnation. In the year 1788, Blumenbach shewed that corpora lutea

tea may exist in the ovaries of virgins *. Cuvier, in his Lectures on Comparative Anatomy, noticed the appearance of cicatrices in the ovaries, without impregnation, even independent of sexual intercourse. These observations corroborated by the experience of subsequent physiologists, have entirely settled this point. The corpus luteum is not positive evidence of impregnation. Excitement of the ovaries, whether from the desire of sexual intercourse, or from unfruitful coition, is sufficient to cause the rupture of the vesicles, and the formation of corpora lutea. Nor does the existence of a decidua in the uterus constitute better evidence of impregnation, since in some instances it has happened that at each period of painful menstruation, the excitement of

* Commentat. Soc. Reg. Scientiæ. Gotting, Vol. ix.

the uterine vessels has determined the formation of a perfect decidua, not to be distinguished from that belonging to an ovum. Morgagni has very accurately described a case of this kind*, and the same fact has been noticed by other individuals. Our opinion in a case of supposed impregnation must be formed from the review of all the circumstances appertaining to the condition of the uterus, ovaries, and Fallopian tubes; and even then we must recollect that nothing short of the actual inspection of the ovum can be regarded as decisive testimony upon the subject.

In the case which has been here related, the changes which the uterus and Fallopian tubes had undergone, would together have afforded strong presumptive evidence of impregnation. These changes, combined with the ap-

* On the Seat and Causes of Diseases, Letter xlviii. Art. 12.

pearances in the ovary, can scarcely leave a doubt upon the subject. Further, the characters of the vesicular body in the ovary, its two distinct coverings, its vascularity and the effusion of blood around it produced altogether in the minds of those who saw it, among whom there were some very competent to judge, the conviction that it was an ovum. The remarkable alteration which the Fallopian tubes had undergone in their internal membrane must be considered as preparatory to the reception of the ovum. An objection to this opinion can scarcely be taken from the circumstance of both Fallopian tubes having undergone this change when there was only one ovum. The harmony of action constantly observed between the two Fallopian tubes would lead us to expect corresponding changes in both of them at the same time.

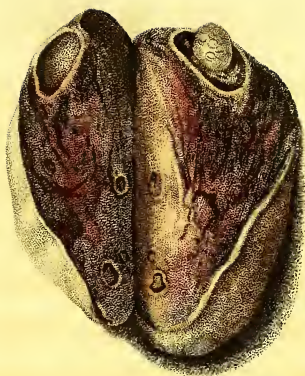
In a paper recently published by Sir Everard Home in the Philosophical

cal Transactions, the occasional detention of the ovum in the ovary is explained by the circumstance of an adhesion taking place between them, such as existed in the case here related. But with deference to this opinion, the difficulty may be noticed of determining whether the adhesion of the ovum to the ovary be the cause or the effect of its detention in that situation, for in the supposition of the ovum from any cause failing at the proper period to burst its way through the ovarian coverings, the natural process would then be, that its outer membrane or chorion should acquire an adhesion to the parts with which it is in contact, so that it may in this way open a communication with the vessels of the ovary ; and derive from them the materials for its growth.

It is perhaps a question whether the effect of the excitement in the ovary of the virgin extends beyond the rupture of the vesicle and the

production of the corpus luteum ; the formation and escape of the rudiment of the ovum, is certainly in this case as yet a subject of doubt. The corpora lutea in the ovaries of virgins may in general be distinguished from those which are the consequence of impregnation by their smaller size. Further, it may be noticed, that in the human subject, the time required for the formation of the corpus luteum varies greatly in different instances, as will be obvious from the examination of several ovaries at the same period after impregnation.

END OF VOL. VI.



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Plate shewing the Ovary & Fallopian Tube,
as they appeared upon Dissection.—

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